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A paper read at the 150th Meeting of the American Institute of Electrical Engineers, New York January 3, 1902, President Steinmetz in the Chair.

METHODS OF ILLUMINATION.

BY LOUIS BELL.

At the risk of being iconoclastic, I am here going to discuss the art of illumination from its human and practical side rather than the technical details of electric lighting, and to compare methods as bearing upon the result rather than in their theoretical relations.

Artificial lighting in some form or other has come down to us from the very dawn of history, but without material change in methods and apparatus until well into the last century. Its resources have been vastly increased within the past seventy-five years and have grown up with the rise of what one may call technical civilization. The development of electric lighting which has taken place during the last quarter century has enriched the art with extraordinary facilities, and it is the bearing of these upon the art in general and upon each other that forms the subject under discussion.

Now, at the very outset, I must confess the profound conviction that as things are at present, we are using the tremendously potent means at our command with a reckless indiscrimination that in the long run profits little to art or civilization. We have contracted in no small degree the modern and peculiarly American vice of megalolatry, if one may venture to coin the word, and estimate illumination like many other things by the magnitude of the display.

It is difficult to realize in this the change that a century or so has wrought. As nearly as we may estimate from the somewhat meager data that have come down to us, the most splendid fete

in the days of Louis Quatorze glittered with less than a fifth the illumination that makes conspicuous the bourgeois tawdriness of a twentieth century function, but who shall care to say that ratio represents a gain to art or a new conception of regal magnificence? Art and civilization do not necessarily gain with added physical resources. Better the rush light that shone on the wine-stained doublet of Will Shakespeare than the electric arc with Mallock and Oscar Wilde.

The truth of the matter is, that with the immensely powerful sources of light now at our command, there is a constant and often reprehensible tendency to turn on more and more light irrespective of quality and of actual needs until the net result is loss rather than gain. We too often forget that illumination is a problem at once physiological and physical, and that by reckless profusion of light it is a perfectly easy matter to key the vision as it were, up to a pitch which demands extraordinary methods to produce the most common-place results.

It is a fundamental physiological fact that the eye works equally well, sees with equal facility, within a very wide range of illumination. True, as the light intensity is reduced, a point is finally reached at which the normal eye begins to labor and strain, picking out details instead of grasping the whole, but it is equally true that if the lighting is carried above a certain intensity the eye reaches a point where it not only gains nothing in case of perception, but suffers from over-stimulation. Between these limits, varying obviously for different tasks, there is no intrinsic reason for increasing or diminishing the illumination except in so far as a reason may be sought in convenience or economy.

Whatever the methods employed, the end of artificial lighting should be to furnish illumination in quantity fully adequate for the conditions to be met, and in quality such as will neither unduly strain the eye nor in any way inconvenience the user. It is the adaptation of means to end that should be sought here as everywhere else. From this standpoint, quality is more important than quantity. It is no easy matter to define the quality of light in any simple terms. Certain qualities, however, must be possessed by any really good light in order that it may not do positive injury to the eye, and may prove efficient visually. The three most important qualities for a practical illuminant are in the order of their importance as follows:

1. Steadiness.

2. Suitable intrinsic brilliancy.
3. Suitable color.

1. *Steadiness.*—The importance of this property cannot be over-estimated, for without it in a rather high degree an illuminant is both impracticable and dangerous to the eye. An unsteady light works evil in two distinct ways, somewhat different in their relation to the physical properties of the light. In the first place we must remember that the eye is provided with a partial defense against unsteadiness in the visual inertia given by the persistence of retinal impressions, and in addition it has the iris working automatically to steady the amount of light which reaches the retina.

A flickering, unsteady light may injure the retina through overworking it by a rapid succession of violent stimulations, or it may tire the automatic mechanism of the iris, or it may do both. The iris is peculiarly a defence against slow changes in illumination. At full aperture it has 15 or 20 times its area at mimimum aperture, or perhaps double this ratio under extreme conditions, but its time constant is large, several seconds, and in an unsteady light it never gets a chance to settle into a position of equilibrium. Visual inertia is a defence against very rapid changes, but not against slower ones, and there is a wide range of frequency between the point at which visual inertia fails to protect, and the point where the iris can give material aid. Within this range, unsteadiness, whether accidental or periodic, may be both distressing and dangerous.

Let us try if possible to get a quantitative conception of this case. The frequency at which flickering in incandescent lamps becomes annoying is a much mooted point, the view of the debater generally depending on what he has to sell. The real question, however, is what variation in amplitude of illumination constitutes an amount which is physiologically serious if periodically repeated. Obviously a change from light to darkness is vastly worse than a small change in brilliancy at the same frequency. Eyes differ in sensitiveness, but in my own case I have found that a variation of 10 per cent. at the rate of one or two maxima per second is very annoying and tiring. This observation was made, by the way, with the potent assistance of a pumping synchronous motor which could give variations in voltage up to five volts on an ordinary incandescent circuit. Retinal per-

sistence is of course a very variable phenomenon, but for a rough approximation, taking this constant at 1/5 second, a 10 per cent. falling off in brilliancy would correspond to about 1/50 second and one would feel entirely safe in saying that sudden flashes 1/50 second apart would cause trouble at once, and, probably owing to frequent recurrence, very serious trouble. In point of fact an alternating arc worked with small current density gives such a succession of flashes, and in spite of a certain amount of thermal stability would be quite impossible at 25∼, and is bad even at 100∼ per second. Incandescent lamps, having greater thermal stability, present smaller fluctuations due to frequency, but are very bad at 15∼ to 20∼, and show fluctuations troublesome to some eyes up to and even above 35.∼

Under conditions of prolonged use, fluctuations considerably less than 10 per cent. may become troublesome as is well shown by the evil effects of flickering gas burners, in which the changes of absolute intensity are small. Here of course the phenomenon is complicated by flickering at the edges of the retinal image, but taking all the facts together I think that one may safely say that any source of light subject to rapid and frequent variations as great as 5 per cent. is unfit for use as an illuminant for work requiring steady use of the eyes. Much greater changes occurring slowly enough for the iris to act with them, or at infrequent intervals can be tolerated without inconvenience.

2. *Intrinsic Brilliancy*.—The brightness of the radiant used in artificial lighting is a matter of great physiological moment. If the image of a brilliant light remains on one point of the retina it may and probably will produce serious over-estimation, and the bruised spot, as it were, may become the nidus of painful and dangerous inflammation. Any oculist can report a long list of serious or destructive injuries due to this cause. On the other hand, if the image wanders over the retina rapidly, it produces in a minor degree the same unpleasant result experienced in coming suddenly from a darkened room into bright sunshine. Especially is this true if the brilliant radiant now and again dodges into the field of vision without giving the iris a chance to act. The fundamental rule of proper illumination is to keep brilliant radiants out of the field of vision. Apart from the mischief done, the iris in the presence of such a radiant shuts up as far as possible, and objects not brilliantly illuminated are seen with great difficulty. Hence the common experience of being

unable to see across an arc light, and the serious difficulty thrown in the way of pilots by search lights unwisely displayed. It is a common experience, too, that shading a brilliant radiant may greatly improve the seeing conditions in a given space while actually reducing the available illuminator. It is almost impossible to get a quantitative measure of such a gain, though it is often quite marked. The ingenious illuminometer of Messrs. Houston and Kennelly, and the tint comparing instrument of Mr. Bijur, another fellow member, both break down here, since they work a shielded eye.

The real criterion of visual usefulness is not the illumination I, but the product $I\ \sigma$ where σ is the effective aperture of the iris, and since this latter factor is a function of the brightest radiant in the field we may properly write the criterion

$$\frac{I}{f(B)}$$

where B is the intrinsic brilliancy of the radiant. This whole subject needs investigation, but like other matters involving subjective facts, it is very hard to get at. We can, however, get an approximate notion of permissible values of B for practical purposes. The experiment of L. Weber on intrinsic brilliancy give a clue to the conditions found in practice. Reducing them to mean values and candle power per square inch, some of his results are as follows, approximately :

Horizontal white card, brilliant sunlight	25
White sunlit cloud	7
Argand burner	6.5
Horizontal white card, dull winter day	0.26

Now there is no doubt whatever that the intensity first mentioned is far too great for comfort, considering the normal eye. It is, in fact, extremely trying. The second and third, most of us would consider rather uncomfortably bright if they were fairly in the field of vision, and all the experience along this line seems to indicate that for radiants within the field of vision at the distances ordinarily met with in interior lighting an intrinsic brilliancy of 5 c. p. per square inch is about as high as it is wise for hygienic reasons to go, and better, rather less than rather more. This indicates at once for the radiants in common use, that they should never be employed within the working field of vision without diffusing shields to keep down the intrinsic brilliancy.

Obviously with the given limit of intrinsic brilliancy, the diffusing globes must be of fair dimensions. With proper diffusion the entire globe surface may be taken as the radiant, and for a first approximation the radius of a spherical diffusing globe should be at least

$$r = \frac{c\,p}{5\,\pi}$$

c. p. being the candle power. With very high intrinsic brilliancies, such as are given by arc lamps, it is hard to avoid the appearance of a nucleus of great brilliancy, and proper diffusion means considerable loss of light. But arcs should be kept for the most part out of the working field of vision, so that this objection is not so serious as might otherwise be the case.

3. *Color.*—It is not necessary that artificial illuminants should conform to a particular color standard, save where color is a matter of prime importance. It is, however true, that the general applicability of a light is very strongly influenced by its hue, and that broadly, strongly colored lights would fail conspicuously in visual usefulness. Quite aside from the matter of color perception, it is the orange, yellow, and green that make up the working luminosity of the spectrum formed by sunlight to the extent of about 80 per cent., and even were a brilliant deep blue or red light produced, it would require far greater energy for the same luminous effect than a yellow or greenish light.

The eye unquestionably does its best work by light approximately like that through which it has been evolved. But sunlight is a very variable quantity. Outside the atmosphere the sun would appear conspicuously blue, but we live and move and have our being inside the atmosphere, and the color of the illumination outside is only of academic interest. On the meridian the sun gives light that is to all intents and purposes white, but as it declines, the atmosphere steadily filters out the blue and violet, until by the middle of the afternoon the entire blue end of the spectrum is down to about half its previous intensity, while the general luminosity has fallen only about 20 per cent. The general color tone is then distinctly yellowish white, and still later in the day when the sun is only a few degrees from the horizon, the blue contingent is practically wiped out, and the light shades off from orange yellow to orange red. Brilliant diffused light, being mixed with con-

siderable sky light, which is noticeably blue, for the very reason that the setting sun is not, is perceptibly richer in blue rays than average sunlight.

For our purpose, the conspicuous fact about natural light is that it seldom or never verges on the blue or violet, and never is tinged with blue-green, green or yellowish-green. Its average tint is slightly yellowish-white, with a tinge of orange late in the day.

A color match made at high noon will not hold when the sun is on the horizon, but memory of exact color value is not much to be trusted, and the knowledge that the tints did match at noon usually convinces the observer at sunset that "the light is poor," any variation being attributed to lack of light rather than to its real cause, change of hue.

As regards artificial lights, generally supposed to fail conspicuously in giving what we are pleased to call "natural" color effects, every one of them, except the enclosed arc, which is strongly bluish, and the Welsbach, which is greenish, can be matched by normal sunlight at some time of the day. Even a Hefner lamp is not redder than a low sun on a hazy day.

The ideal artificial light from a purely utilitarian standpoint should represent average sunlight. So far as artistic results are concerned, from the present canons of human taste, a light weaker in the blue and green is to be preferred. Taste may some day be evolved to a point where a thallium-green illumination will be considered becoming, but we are not there yet.

So far, then, as color is concerned, one can plan for artificial illumination with reasonable freedom of choice. It is by no means clear even that it is always desirable to employ white light for color-matching, unless one can guarantee that the match will be viewed only by white light. An enclosed arc worked with rather small current and untinted globes will distort average or normal color values quite as much as will gas-light. Hence, the importance of enclosing shades which correct color as well as diffuse light.

Classes of Illumination.—One may broadly divide illumination into (1) *General* and (2) *Directed*. Each class has its own relations to the requirements for quantity and quality, and each may be further subdivided. But so far as methods as to the choice of illuminants are concerned, there is an important distinction between light intended to be generally diffused and light

intended to facilitate particular operations at particular points. It is in the adaptation of methods to ends that the *finesse* of the art appears.

If by some new invention the energy required to produce one candle power in illuminants of all classes could be cut down to one-half its present value, the world would be in an uproar over it. But it is well within the truth to say that skilfully treated illumination will effect at least an equivalent saving, or will give vastly better results for the same expenditure of energy. Highly efficient radiants are *per se* very desirable, but if efficiency is gained at the loss of other important qualities, it may be dearly bought. Illumination, visual usefulness, is the thing to be sought. On this basis it has been often maintained with some show of reason that the working unit by which practical lighting should be measured and practical radiants judged is illumination rather than intensity.

Illumination vs. Intensity.—In favor of using the measured illumination as the criterion by which to judge sources of light, it may properly be said that illumination rather than light intensity is the thing which is practically important. Illumination is the object of artificial lighting, and it is a matter, from this standpoint, of no particular consequence whether the radiants have one or another particular intensity so long as the result is achieved.

On the other hand, I have already shown that the real criterion in artificial lighting is not the illumination as measured in certain conventional ways, but what I have called "visual usefulness"; *i. e.*, the illumination as modified by the way in which the radiants are applied. Moreover, illumination is an extremely difficult thing to measure with precision. There are difficulties enough in ordinary photometric measurements, even when pursued with the aid of the best modern apparatus. The difficulties in the way of measuring illumination in terms of the lux or candle foot at various points in the space that is artificially lighted, are ten-fold greater. It is photometry with all the conditions against precision, and even with the illuminometer of our confreres Houston and Kennelly, as good an instrument as yet devised for the purpose, the mean error of a single observation is stated by the inventors to be about 10 per cent. It and all of the instruments of its class for the purpose of measuring illumination have proved, as a matter of experience, to be both

rather slow and rather inaccurate; hence, such measurements are in themselves unsatisfactory as means of passing final judgment upon commercial lighting.

It should further be said that the instruments for measuring illumination as such, seem to be afflicted with peculiarly large personal errors. Some observers, otherwise careful and trustworthy, are unable to get coherent results from them at all, while occasionally one acquires a knack in their use so as to obtain very consistent results, which, however, are more than likely to vary widely from the results obtained by another successful observer.

Aside from this, even were it possible to make measurements of illumination with precision, illumination is not in the last resort the thing which we are really after, which is the power to see, *i. e.*, visual usefulness, and this involves physiological quantities of which it is almost impossible to take physical account.

Moreover, the testing of lighting by illumination, pure and simple, involves the art of placing radiants, and while commercial measurements by illumination might tend to develop this art they will also afford almost unique facilities for meeting the letter but not the spirit of the specifications, facilities which are only too great in the case of even intensity measurements upon commercial illuminants. which are susceptible of determination to a fair degree of accuracy. Hence, in view of all these facts, I believe that, attractive as measurement of illumination, or still better, visual usefulness, may be in theory, in practice it will prove far better to fall back upon intensity measurement on which the illumination measurement must in the last resort depend.

As a rule, I have observed that there is a tendency to favor measurements of illumination chiefly on the part of those engaged in exploiting illuminants that cannot satisfactorily stand comparison on the basis of intensity.

We must then, I think, judge commercial illuminants so far as their quantitative relations are concerned by intensity measurements, leaving their qualitative relations—some criteria regarding which have already been suggested—for consideration on their merits.

The Intensity of Illuminants.—The fundamental measurement then on which one must base all comparison of methods of

artificial illumination is the measurement of the intensity of the radiant.

I have neither time nor disposition here to bore the INSTITUTE with any discussion of the teconique of photometry. The general methods and apparatus, however, are worth some considerable notice.

To my mind the chief desideratum in dealing with matters of artificial lighting is the settling down of practice on some conventional basis, as regards a standard of light. Working standards may be easily constituted from incandescent lamps, but to obtain a primary standard reproducible with a good degree of precision, and answering reasonable requirements as to color and steadiness is a very different matter. The literature on the subject is large, and our INSTITUTE committee has added not a little of value to researches bearing on the subject, but up to date there certainly is not in general use a standard which anywhere nearly meets requirements. The Violle platinum standard, which was a laudable attempt to obtain a standard light which in a general way would fall in line with the c. g. s. system, may be said to be practically non-existent. It has not proved to be properly reproducible, and the experience of most of those who have tried it, is, that it is a ticklish and uncomfortable sort of standard to deal with; consequently, its sub-multiple, the *bougie-decimale*, must be regarded as a species of luminous phantasm so far as present practical developments are concerned, and with the *bougie decimale* should tentatively be classified its derivative, the *lux*. Lux is the conventional name for the fundamental unit of illumination, but so long as it is referred back to the *bougie decimale*, which is not a practical standard of intensity, it must remain in a species of scientific limbo from which in due time I trust that it may be freed.

As practical standards we have in use in the scientific and commercial world, the English standard candle, which is the only one having in this country a legally recognized existence; the Hefner-Altneck lamp adopted by the International Congress of 1896, most thoroughly investigated by the Reichsanstalt and scientifically in good repute; the German candle, perhaps, now tending to pass out of use; the French Carcel lamp and the Vernon-Harcourt pentane lamp now developed in a 10 c. p. size.

The relations between these various standards of intensity are only approximately known, each fresh investigation of the sub-

ject tending to give a new set of values differing very materially from the last.

The English candle we have all used and, I think, have cordially disliked. It is not very accurately reproducible, although conveniently so. It is not steady to a suitable degree. It is rather small for work with commercial radiants and only fairly good in color. It is, however, the legal standard in this country, and so long as it remains so must be treated with a certain degree of respect.

The Hefner-Altneck lamp, on the other hand, is quite accurately reproducible and fairly steady. It is, however, open to two objections so grave that they must inevitably ere long cause its rejection as a general standard. These objections are its very small intensity and its extremely bad color; indeed, its use as a standard involves the problems of color photometry in a somewhat aggravated form. Any one who has often attempted to balance the Hefner-Altneck lamp against an incandescent for example, will realize that although consistent results can by extreme care be obtained, the comparison is a distinctly troublesome one, and not what one would care to permanently depend upon as the basis of scientific measurement. It is essentially a difficult comparison.

The Carcel lamp is somewhat better in the point of reproducibility and steadiness than the English candle, and not as good as the Hefner-Altneck lamp. Its intensity is decidedly more convenient, however, and its color is better than that of the Hefner-Altneck lamp. It can hardly be considered, however, as a standard which is likely to be used in an international way and certainly is without standing in this country.

The Vernon-Harcourt pentane standard—the present legal standard in London—deserves, I think, respectful consideration. It has recently been carefully looked into by the committee of the American Gas Lighting Association with somewhat encouraging results. This committee found a very satisfactory degree of constancy under rather widely varying conditions. The point of greatest interest in its report was, that pentane of practically constant quality is commercially obtainable, so that so far as the material consumed goes, this standard would seem to be satisfactory as the amyl-acetate one as regards reproducibility. The constancy appears to be satisfactory, and the color and magnitude of the standard are such as to give it a strong

claim for consideration. It has not yet been investigated so completely that a final estimate of its desirability may be formed, but I am strongly inclined to the opinion that a standard of as high intensity as this, is very desirable, and that this particular standard is worthy of very serious consideration.

I have passed by in this brief enumeration the Methven screen which is in considerable and successful use as a secondary standard, but concerning which the consensus of opinion seems to bear out the view, that in point of reproducibility and constancy it leaves something to be desired.

Such then is the state of things as regards standards, and the unsatisfactory nature of the situation is not open to debate. The most important step to be taken in the reduction of illumination to a practical condition, is the general acceptance of a primary standard even as a tentative matter, so that instead of various investigators working with standards bearing only approximately known ratios with each other, they shall, at least, deal with the difficulties and errors involved in the practical use of a single standard. At present we have results given in candles, decimal candles and Hefner units, and we have to look carefully at the investigator's preface to discover to which of these he has temporarily pinned his faith.

As derived units of illumination we have the lux, in theory based on the decimal candle, and occasionally passing into variants, like the candle-metre, using the English candle, or the Hefner unit-metre in its turn.

Still apart from these we have the candle-foot, which falls in logically enough with our English measurements, but can never be satisfactory as an international standard. Considering the uncertainties at present existing in the fundamental units in the matter of intensity, it is small wonder that the derived units are likewise in a bad way. The logical derived unit, which has the greatest importance in a practical way, is the unit of luminous flux, called lumen in the tentative nomenclature of the Congress of 1896. The conception of light out-flowing from a center and all available light as a quantity based upon this conception, is a most useful one. The total light given by any radiant must evidently be expressed in some such way as this, in order properly to take account of its sometimes very great variations in different directions.

As regards the much-mooted question of a conventional direction of measurement in comparing radiants, it seems to me that the application of a little common sense simplifies the problem wonderfully. If light emitted in all directions from a radiant point may be useful for the purpose of seeing—a proposition which scarcely admits debate—then the total luminous flux is the thing which ought to be measured. If for a particular purpose, under particular circumstances, concentrated light in one direction is desirable, then for that purpose and under those circumstances, light in that direction should properly be measured. It is the case of general illumination on the one hand, and directed illumination on the other.

The light from various radiants is distributed in a very diverse fashion. The commonest distribution is that of a more or less flattened ellipsoid, with cusps more or less pronounced at either extremity of the axis. Whatever the peculiarities of the distribution, the total light-giving power of the radiant as a whole is settled once for all by its mean spherical value. There is no other method of measurement which gives even an approximate value for the total luminous power which that particular radiant furnishes. In the last resort, this total luminous radiation is the important scientific problem, and hedge it about as we may with certain conditions as to its distribution, in comparing the efficiency of radiants as regards light-giving power, mean spherical intensity is the only ground in common upon which all can stand.

As Chairman of the Committee of the National Electric Light Association for the rating of incandescent lamps, I have wrestled for the past five years with the intricate and annoying problem of finding for commercial convenience, if possible, some equitable and simple method of applying a common test to radiants differing even so little in their distribution as do incandescent lamps, and after canvassing the whole question and examining a vast amount of data on the subject, after struggling to escape from the details of the somewhat intricate mean spherical measurement, I am compelled to fall back upon it as the court of last resort in matters of illumination.

I shall speak later of some of the annoying practical considerations that have entered into this conclusion. The mean spherical standard, however, is the common basis from which all radiants can fairly meet for a judgment of their total light-giving power

and for a comparison of the total luminous energy which they furnish, as related to each other, and as related each to its own consumption of kinetic energy. Many objections based upon so-called practical considerations have been raised to this primal rating, but they have been and are uniformly raised by those exploiting illuminants, which can be measured in some other way more advantageously to themselves.

It is perfectly natural from the commercial standpoint for one to desire that his particular radiant should be measured in its direction of maximum candle power. It reminds me of the early arc lamp salesmen each of whom vociferously lauded his particular system before a board of aldermen as furnishing lights of 8,000 candle power, and when asked for an explanation promptly replied that his lamps were placed at the corners of two intersecting street and gave 2,000 candle power up and down each street. If that was not 8,000 candle power, he would like to be informed why not.

There likewise have been, not infrequently, objections raised to using a candle power or similar rating at all, or indeed the falling back upon photometric measurements of any kind, but in all such cases it is entirely safe to assume that a mean spherical measurement of the objector's particular illuminant on a purely photometric basis would disclose very potent reasons for his disinclination for having such a measurement made.

Practical Illuminants.—As electrical engineers in dealing with problems of illumination we practically make use of the following classes of illuminants: Open arcs, enclosed arcs, and incandescent lamps of various types and efficiencies, to which we must now add the "Nernst" type of lamp.

We may, perhaps, look forward in the future to luminescent lamps of one type or another, but at present these, I regret to say, have no practical bearing upon the art of illumination. These available radiants differ in all the properties which go to make up a useful illuminant, and possess, each of them, vices or virtues more or less important with respect to their adaptability to the solution of various practical problems in artificial lighting. It is perfectly safe to say that no one of them has shown, up to the present, such conspicuous superiority in all particulars, as to give any logical reason for adopting it to the exclusion of the others. They vary widely in total power, in intrinsic brilliancy, in steadiness, in color and in efficiency, and all of these properties have to be borne in mind in adapting them to practical business.

General Illumination.

By general illumination I mean such illumination as is ordinarily required for illuminating considerable spaces in-doors or out.

General illumination is intended—so far as in-door use is concerned—to give light for the ordinary avocations or amusements which people carry on after nightfall, and must be proportioned in intensity and color to the requirements of these conditions.

General illumination out-doors is for similar general purposes, but the requirements for it differ from those for in-door lighting, by reason of the fact that general illumination out of doors is not for the purpose of enabling one to do any particular work, least of all work requiring a strong light, and consequently is usually very much less in amount than general illumination in-doors. Moreover, inasmuch as the chief use of illumination out of doors is to enable people to get about conveniently, or at the most to handle rough work expeditiously, its quality in color, brilliancy and even in steadiness, need not come up to the mark which is at once necessary if one attempts to read or to carry on ordinary work by the light in question.

In recent times the tendency has been to use arc lamps for all general illumination out doors, reserving in-door work for incandescents; a task which it has shared in varying proportions with ordinary gas, or gas in some of the modified mantle burners. The chief merits of the arc for out-door work are cheapness and power.

The old open arc, which until the last few years has done the bulk of general out-door lighting, possessed the merit of giving at a high efficiency a very powerful light at a moderate price. Its conspicuous demerits were great unsteadiness and intrinsic brilliancy so high that it either had to shaded or else installed so high above the ground as to be practically out of the field of view except from a distance.

The old open arc was, as all of you know, of two general types: the high voltage arc, *i. e.*, the ordinary type at about 50 volts, and the old low-tension arc of about 35 volts pressure between the carbons; the amount of current used in the two being practically inversely to the voltages employed.

The former type was the one which survived the earlier years of electric lighting, and the so-called 2,000 power arc, taking about 9.5 amperes, was generally regarded as standard for out-

door work. In point of fact, this open arc was and is the most powerful radiant practically used for exterior illumination. Its great intrinsic brilliancy was always an objection to its use in-doors or near the ground, and its distribution of light—said distribution being largely in a narrow zone about 45 degrees depressed from horizontal—was not such as to answer successfully the requirements of street lighting. The consumption of carbons was rapid, and the lamps seldom burned steadily. In-doors the case for such arcs was much worse. They were too bright, too irregular in distribution and far too unsteady for such illumination. From a mistaken notion of economy, also, they were generally installed in too few numbers, each arc replacing about twice the number of gas jets which it ought to have replaced, with the result of giving an unequal illumination with very sharp and dense shadows that proved extremely annoying to the eyes, and the visual usefulness of which was extremely limited.

With the growth of constant potential circuits special open arc lamps were devised, which overcame in a measure certain of these difficulties, particularly the unsteadiness. The difficulties of too great brilliancy and bad distribution remained, however, with the open arc even in this improved form.

Abroad it has been and is the practice to use two and sometimes three constant potential open arcs in series across the ordinary lighting mains, and owing to the fact that carbons of a very high quality are available in Europe at a low price, in point of steadiness the Continental type of open arc has generally proved very satisfactory, while the installation of two or three in series so greatly increases the output of light for a given expenditure of energy when working on a constant potential circuit, that one can afford to enclose the arcs by dense diffusing globes, which almost entirely obviates the difficulty of too great brilliancy.

In this country, as you all know, the recent tendency has been entirely toward the enclosed arc. The great economy of carbons and of labor, together with the steadiness of the light and the improvement of the distribution by the lengthened arc and by the diffusing inner globe, have operated to bring such arc lamps into constantly increasing use both in-doors and out. It must be said, however, that enclosed arcs have one fault which may become and often is very serious, *i. e.*, a tendency toward an exaggerated bluish white or even violet hue, which makes the illumination derived, average as bad in point of color as in any known artificial illuminant.

A tale is current in the profession, of a certain grocer in a New England city, which shall be nameless here, who exposed for sale in front of his store a large box of cranberries, only to have them admired as the biggest blueberries of the season when evening came and the enclosed arc hanging in front of his store put in its work upon the cranberries.

This is but a slight experience of a condition which may exist and which seriously hampers the usefulness of certain kinds of enclosed arcs when used in doors.

For most out-of-door work the bluish tinge of the enclosed arc lamp is not an objection. Its steadiness is admirable and its distribution is so very much improved that for street lighting the uniformity of illumination is far greater than that attainable by the earlier form of open arc.

Still more recently the enclosed alternating arc has come into wide use for street lighting and to a less extent for in-door work. The distribution of light from this radiant is even somewhat better than that from the enclosed direct current arc. The intrinsic efficiency of the alternating arc, however, is distinctly less than than of the direct current arc and compensating advantages have to be sought in its somewhat better light distribution and in the somewhat better efficiency of the apparatus by which the energy is delivered to the lamps.

I may not here enter into the question of alternating current vs. direct current for arcs for street lighting, since it is one which could be threshed away at for the entire evening without saying half that could be said. Suffice it to say that if one uses the alternating arc, it is in my opinion necessary to provide somewhat more energy per lamp than in the case of the direct current arc, either open or enclosed. The efficiency of the lamp for street lighting may be expressed in gross candle power, in illumination within a certain specified radius, and by the minimum illumination at the extreme distance between lamps.

So far as absolute brilliancy goes, there is no question that the old 2,000 candle power open arc gives a greater luminous flux than any of the improved arcs which have replaced it. Its distribution of light, however, is open to serious objections on the score of general economy, for the centre of the zone of the maximum of illumination falls at a distance from the foot of the lamp post equal to the height of the lamp above the pavement, and within and without this zone the light falls off somewhat rapidly.

Both the direct current and alternating current enclosed arc throw the maximum of light nearer to the horizontal and hence produce a larger and better illuminated area.

If the open arc could be used with a proper diffusing globe, such for example as the Holophane globe, sometimes, but rarely used on arc lamps, it could undoubtedly duplicate the more uniform distribution of the enclosed arcs at increased efficiency, but any efficient plan for redistributing light in this way means working with the arc in a fixed focus; *i. e.*, it means practically a focussing lamp, if the carbons are rapidly consumed as they are in open arcs. The increased cost of the focussing lamp has so far debarred its use in this country, so that the open arc has been denied the advantage of such fixity of the radiant point as would be desirable.

We judge the illumination of a street by two things; first, by the light actually delivered at the surface of the ground; second, by the general illumination produced along the street.

A light which merely renders luminous the dull surface of the pavement, leaving the houses and objects along the street in comparative darkness would not in the long run prove to be a satisfactory means for lighting a street; hence, one must take into account the general light delivered as well as that furnished on the plane of the ground. Light delivered much above the plane passing horizontally through the lamp is of comparatively no effect in improving general illumination, unless the lights happen to be in narrow streets bordered by unusually high buildings. A view of any large city from a considerable distance at night gives a vivid idea of the waste of luminous energy which goes on. Sometimes for many miles the lights of a city are visible by the reflection upon the clouds and diffusion from the floating particles in the air above the city.

As to the practical lights used for street lighting, it is hard to suggest a fair and adequate method of rating. Out door work differs radically from in-door work in one very important particular; *i. e.*, in the aid which is received from diffusing surfaces. Out-of-doors there are practically no diffusing surfaces of any importance on which the light falls, except the ground when snow-covered in winter. In-doors the aid received from the diffusion is very important, as we shall presently see; hence out-doors, rays thrown much above the horizontal are comparatively ineffective. Rays thrown directly downward need not be strong

in order to produce adequate illumination at so short a distance from so powerful a radiant. The best test of relative effectiveness would probably be the zonal candle power of the radiant over a specified number of degrees below the horizontal. This is obviously a somewhat unusual and difficult method of testing and the commoner rating by lower hemispherical candle power, which has come considerably into use, is reasonably fair and convenient. The best test of all is the general visibility through the region to be illuminated, not the minimum illumination expressed in candle feet, or the average illumination over a zone, or the apparent brightness of the radiant at a fixed distance, but rather the general impression of useful working light as it forces itself on the attention of an observer used to judging relative illumination. This is a thing for which one could hardly draw a specification, but which is distinctly a function of the intensity of the light given and its distribution. It is not at all a case where photometry would not give a useful result, but rather a case where if one applied the somewhat slow and troublesome photometric processes one would find a condition of affairs which always appeals at once to the educated senses.

Speaking in a general way on this basis, experience shows that the so-called 2000 candle power arcs can be replaced lamp for lamp with enclosed continuous current arcs taking between 6.5 and 6.75 amperes with, upon the whole, distinct improvement to the general lighting, especially in the lessened intrinsic brilliancy of the radiant. The minimum illumination between lamps will remain about the same; the bright zone disappears; the average illumination over the area is somewhat less, but the visual usefulness—which, after all, is the vital test of general illumination—is distinctly greater with the enclosed arcs, by reason of their less dazzling impression.

With the alternating arcs conditions are not quite so favorable. The 6.5 ampere alternating arc is certainly not the equivalent for street lighting purposes of either the so-called two thousand candle power open arc, or the enclosed direct current arc taking equal amperes with the alternating arc. The latter throws out a little more light far toward the horizontal, but from the effect produced the general usefulness of the light as an illuminant for streets is distinctly inferior. When pushed to about 7.5 amperes, however, the enclosed alternating arc is fully up to the enclosed direct current arc of 6.5 amperes, or thereabouts.

Either of the enclosed arcs at 6.5 amperes is superior to the so-called 1,200 candle-power open arc. I am compelled, however, to say that the series enclosed arcs, taking in the vicinity of five amperes, are very greatly inferior to those of the ordinary ampereage, and in my opinion, a five ampere direct current enclosed arc is by no means the equivalent for practical illumination of the old 1200 candle-power open arc. With reduced ampereage, both direct and alternating current give a light very strongly bluish and very much inferior in practical visual usefulness to the whiter light of the open arc or the enclosed arc carrying relatively high currents. There has recently been a fairly successful attempt to combat this by the increased density in the carbons, *i. e.*, by using in these lamps with reduced current carbons having considerably less cross section than in the more powerful lamps. This increased density in the carbons produces a salutary effect upon the color of the lamp and renders its use much more satisfactory, although at the cost of somewhat more trouble in renewing carbons.

There has been a great deal of discussion regarding sub-dividing the lights for street illumination, replacing, let us say, 7.5 ampere alternating arcs by an increased number of 5 ampere arcs. This sort of replacement obviously tends to increase the minimum illumination, to lessen the dark spaces between lamps and to give a somewhat lower maximum.

On the other hand, unless the smaller lights are worked with increased current density of the carbons, the quality of the light will be decidedly inferior. Its visual usefulness will also be reduced so as in part to counteract the gain from increased minimum brilliancy of illumination. The lamps also become more numerous, more difficult to take care of, and more costly to install, so that it becomes a practical question for each station to settle for itself on the basis of its costs.

This process of sub-division cannot, however, be carried on indefinitely without getting into trouble from decrease of the maximum illumination; in other words, there will be sufficient light on the street at all points to see to get about, but the general illumination of the street will seem to be and will be comparatively dim. There are cases, as when streets are much shut in by trees, when the ordinary incandescents can be applied with very excellent results, but unless used in great masses they are rather unsuccessful in duplicating the general brilliancy produced

by arcs. Between arcs and incandescents proper in point of power, intrinsic brilliancy and efficiency, comes the Nernst lamp, recently introduced. It is too early as yet to speak of its practical usefulness for street lighting, but if its life proves to be satisfactory and its brilliancy holds up reasonably well during its life, I see no reason why it should not usefully meet with many of the requirements of a considerably sub-divided street illumination.

Enclosed arcs and incandescents have the obvious advantage of great steadiness, so that they will probably more and more completely displace the old-fashioned open arc for street lighting as for many other purposes. It may be interesting to note that, in round numbers, it takes in the vicinity of 5 k. w. per mile of street to give a reasonably good illumination.

For general illumination in-doors the requirements are radically changed from those of out-doors. Steadiness and good color form very important considerations. The intrinsic brilliancy of the radiant, if it has a high intrinsic brilliancy, must be cut down, as has already been pointed out, by diffusing globes and shades. In a general way the power of the radiants should, for conditions of greatest usefulness, be proportioned to the area to be illuminated. One could illuminate, for example, a circus tent, an armory, or a similar large space with incandescent lamps, but the probabilities are strong that arcs would on the whole answer the purpose better. Contrariwise, one does not ordinarily wish an arc lamp or anything of corresponding brilliancy in one's office. Very brilliant radiants are so objectionable in-doors that the use of shades and globes is imperative, whatever the illuminant chosen. All electric illuminants have intrinsic brilliancy so high as to forbid for hygienic reasons their installation unshaded within the field of view. Even incandescents should be shaded or should have ground or frosted globes, if anywhere within the working field of vision. As has already been pointed out, a diffusing globe on the brilliant radiant improves visual conditions by lessening the glare more than it injures them by reducing the absolute amount of light which gets away from the radiant. Open arcs, as made and used in this country, are generally too unsteady to be worth considering for interior illumination. About their only useful function is in the inverted form, when used for lighting a room by pure diffusion from the walls and ceiling. In every case of interior illumina-

tion diffusion is an important factor in the amount and quality of the illumination. In very high rooms with dark ceilings, or in rooms deliberately blackened, like photometer rooms, the illumination depends chiefly on the light received directly from the radiant. In all ordinary enclosed spaces, however, the illumination measured as such is from one and a half to three times the illumination which can be accounted for by the direct radiation. It is a perfectly easy matter to test this by placing close in front of a powerful radiant a small opaque disk and observing the very considerable illumination within the shadow of this disk due to the light diffused from the walls and ceiling. The shadow may appear dense, but there will be, nevertheless, a considerable amount of working illumination even within the shaded area. The presence of this diffusion has two very important effects.

In the first place it lessens the energy needed for the radiant to produce a reasonable amount of general illumination.

Secondly, it renders the distribution of light from the radiant practically a matter of indifference, providing this distribution is not eccentric in some remarkable fashion.

Both of these facts are of great practical importance in planning illumination, both as to kind and amount. Further than this, the color of the diffusing surface often has an important bearing on the general color value produced by the illuminant used. If one desires to judge colors as they would be judged in daylight, it is necessary not only that the radiant itself should give light near to white, but that the diffusing surfaces, which co-act with it in producing the general illumination, should not be strongly colored.

A well-shaded open arc gives a close approximation to daylight color values. Next to it comes the Nernst lamp and incandescent lamps worked at high efficiency—three watts or thereabouts per candle. Next to these come gas jets of the ordinary kind and enclosed arcs worked with fairly large current; the former erring somewhat on the side of yellow and the latter on the side of blue. The obvious moral is, when using enclosed arcs provide at least one globe with a slightly yellowish coloration, which reduces the enclosed arc to a parity with the open arc in the matter of color values without serious loss of brilliancy.

Finally at the foot of the list come Welsbach lights and low current enclosed arcs. Neither of these should ever be used where color values are an important consideration without elaborate shading to suppress the dominant green and blue tinge, of the respective lights. Since all of these radiants should be shaded for interior work, it is easy to apply such corrections for color by the use of tinted shades, although in some cases the loss of light by this procedure might prove objectionable.

For all this work of general illumination in-doors, the quantity which determines the total useful luminous effect is, with rare exceptions, the total luminous flux and not the flux in any one particular direction. It is the total luminous flux that determines the amount of light available, both for direct illumination and for the diffusion that is scarcely less important. The ordinary observer has no conception of the efficiency and artistic value of diffusion in securing general interior illumination.

Those of you who chanced to attend my lecture early last year before the New York Electrical Society, will perhaps recollect the wonderfully effective diffused illumination, thanks to Mr. Lieb, in whose precinct the lecture was given, it was possible to secure by the use of 32 c. p. incandescents in the frieze around the upper part of a high hall, where practically the whole diffusion came from the ceiling.

Radiants which, on the score of high intrinsic brilliancy or even bad color, would scarcely be permissible, for direct use can be used with great effect in producing a purely diffused illumination. I lay stress upon the importance of diffusion, because it not only wipes out the sometimes annoying differences of distribution between different radiants, but also because the development of diffused lighting gives promise of obtaining artistic results, which can hardly be secured in any other way.

A paper presented at the 160th Meeting of the American Institute of Electrical Engineers, New York, January 24, 1902. President Steinmetz in the Chair.

THE RELATION OF ENERGY AND MOTOR CAPACITY TO SCHEDULE SPEED IN THE MOVING OF TRAINS BY ELECTRICITY.

BY CARY T. HUTCHINSON, PH. D.

The determination of the energy and power necessary to make a certain schedule speed from start to start under various conditions of initial accelerations, braking and use of the "motor curve" is a matter of much practical importance. A number of articles discussing special cases have been published. This paper gives a general solution of the question involved in the movement of a body from rest to rest with velocity varying as shown in Figs. 1 and 2—these two figures illustrating the practical problem of train acceleration. The solution here given covers all cases that can arise in ordinary practice of train movement. It affords a ready means of determining the energy, the power and the losses for any schedule speed, over a course of any length with any initial acceleration, tractive resistance or braking effort, and with any desired use of the "motor curve."

The solution is partly analytical and partly graphical; it may be divided into three stages: (1) The determination of the elements of Fig. 1 to give any schedule speed; this velocity-time (v-t) curve I shall refer to as the "type curve." (2) The determination of a general tramway motor curve sheet and the deduction from this curve sheet of several dependent curves; and (3) The application of the general motor curves to the type curve of Fig. 1 to obtain a general solution of the (v-t) curves of

Fig. 2. Each case of Fig. 2 is reduced to a case of Fig. 1 by applying a correction to Fig. 2, represented by the area M A N, by which it is reduced to a corresponding case of Fig. 1.

Throughout the following units are used:

Velocity, in miles per hour.
Acceleration, in miles per hour per second.
Distance, in feet.
Time, in seconds.
Weight, in pounds.
Power, in kilowatts.
Energy, in watt-hours.

These units are related as follows:

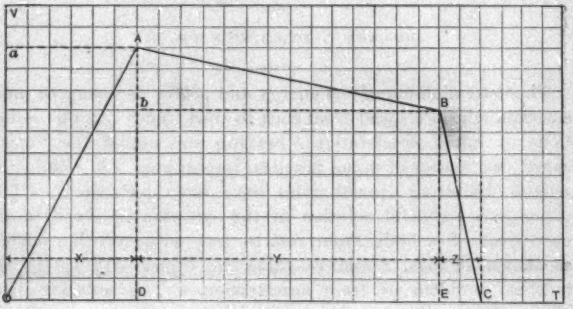


FIG. 1.

Miles per hour (mph.) = 0.682 feet per second.

Miles per hour per second (mph./sec.) = 0.682 feet per second per second.

Kilowatts (kw.) = 9.1×10^{-5} w (mph./sec.) (mph.)

Watt-hours (wh.) = 1.25×10^{-5} w $(\text{mph})^2$.

And, therefore,

Kilowatts per ton (kw./ton) = 0.182 (mph./sec.) (mph).

Watt-hours per ton (wh./ton) = 2.5×10^{-2} $(\text{mph})^2$.

In this system "g" the acceleration of gravity is 22; the ton is uniformly 2,000 lbs.

Referring to Fig. 1, suppose a train to move at constant accel-

eration from rest to velocity D A in time O D, then with constant retardation to velocity E B in time D E, and finally with constant retardation from B to C in time E C; the distances travelled in the time O D, D E and E C are represented by the areas O A D, D A B E and E D C, respectively, the acceleration by the inclination of the lines to the horizontal.

Let

a = Acceleration along O A = D A/O D
b = Retardation " A B = A b/E D
c = " " B C = B E/E C
x = Time for O A = O D
y = " " A B = D E
z = " " B C = E C

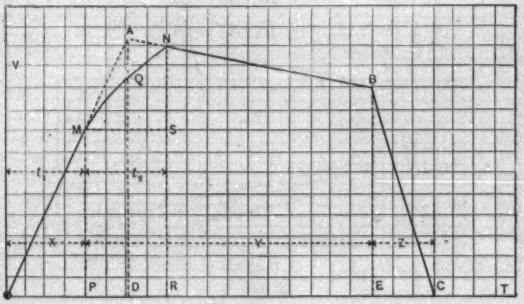


FIG. 2.

s_1 = Distance for O A = Area O A D
s_2 = " " A B = " D A B E
s_3 = " " B C = " E B C
T = Total time = O C
L = " Distance = Area O A B C
V = Average velocity = .682 L/T

Then these three equations hold:

Velocities: $a x - b y - c z = 0$ (1)

Times: $x + y + z = T$ (2)

Distances: $a x^2 - b y^2 + c z^2 + 2 a x y = 2 \times .682 L$ (3)

The work of solving these equations is tedious; I shall give only the results and in a simplified form:

Put

$$A = V/T = .682\ L/T^2 = 1.47\ V^2/L \tag{4}$$

$$2\,A_0 = a\,c/(a+c) \tag{5}$$

$$2\,M = (a+b)\,(a+c)\,(c-b)$$

$$K_1 = (c-b)/M^{1/2}$$

$$K_2 = (a+c)/M$$

$$K_3 = (a+b)/M^{1/2}$$

Where

$$K_1 + K_2 + K_3 = 0$$

Then will

$$x/T = 2\,A_0/a - K_1\,(A_0 - A)^{1/2} \tag{6}$$

$$y/T = K_2\,(A_0 - A)^{1/2} \tag{7}$$

$$z/T = 2\,A_0/c - K_3\,(A_0 - A)^{1/2} \tag{8}$$

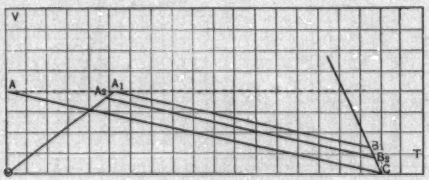


FIG. 3.

And

$$a\,x/T = 2\,A_0 - a\,K_1\,(A_0 - A)^{1/2} \tag{9}$$

$$b\,y/T = b\,K_2\,(A_0 - A)^{1/2} \tag{10}$$

$$c\,z/T = 2\,A_0 - c\,K_3\,(A_0 - A)^{1/2} \tag{11}$$

These three equations are the general solution of the problem in kinematics illustrated in Fig. 1; that is, the motion of a particle from rest to rest, with three different accelerations. The only term in the equations containing the average velocity or the length is A; all the other terms are functions of the three accelerations. This quantity A then determines completely the schedule. All schedules having the same value of A will be accomplished by the same accelerations in the same proportional times, that is, the same fraction of the total time will be occupied

in the three phases of the movement. This quantity is then the independent variable and is of utmost importance in this discussion; it deserves a specific name, and for want of a better I shall call it the "THROUGH ACCELERATION," and denote it by A. All schedules with the same "through acceleration" but with different V and T, are represented by similar figures—the scale only being different.

These equations show that the maximum through acceleration for given values of (a), (b) and (c) is equal to A_0, and that this will occur when $y = 0$; that is, when there is no retardation along A B, or, in electric parlance, "no coasting."

Three special conditions of importance are readily determined:

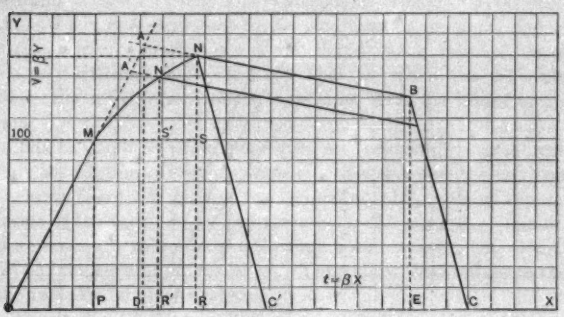


FIG. 4.

(1) Infinite acceleration, $a = \infty$

$$x/T = 0$$

$$y/T = \left(\frac{c - 2A}{c - b}\right)^{1/2} \tag{12}$$

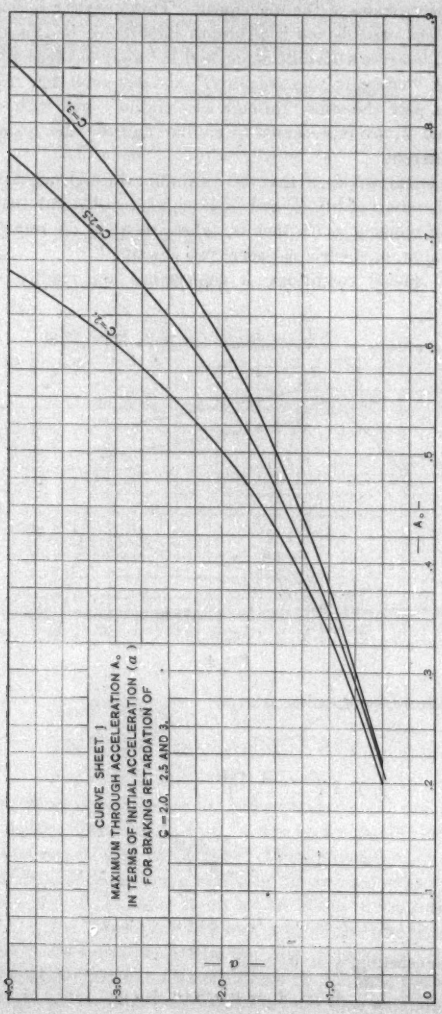
$$z/T = 1 - \left(\frac{c - 2A}{c - b}\right)^{1/2} \tag{13}$$

and

$$a\,x/T = c - (c - b)^{1/2}(c - 2A)^{1/2} \tag{14}$$

(2) No coasting, $y = 0$

$$A_0 = A = a\,c/2\,(a + c) \tag{15}$$


CURVE SHEET 1
MAXIMUM THROUGH ACCELERATION A_o
IN TERMS OF INITIAL ACCELERATION (a)
FOR BRAKING RETARDATION OF
$C = 2.0$ 2.5 AND 3.
$C=3$
$C=2.5$
$C=2$
$-A_o-$
$-v-$
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1.0
2.0
3.0
4.0

(3) No braking, $z = 0$

$$a\, b/2\, (a + b) = A$$

$$x/T = b/(a + b) \qquad (16)$$

Equation (15) determines the maximum through acceleration, and equation (16) the minimum through acceleration; all possible values lie between these two.

Equation (14) gives the maximum velocity for infinite initial acceleration; this is the least maximum velocity for any value of A greater than $b/2$. For values of A less than $b/2$ the maximum velocity for infinite acceleration is the greatest possible maximum. This seems abnormal, but that it is correct can be seen readily from Fig. 3, where O A C shows the curve for $A = b/2$ and $a = \infty$. Since A is represented by the area of the triangle O A C, it is clear that for an acceleration O A_2 A_1 the area O A_1 B_1 C is greater than O A C, and that at some point A_2 the area O A_2 B_2 C will be equal to O A C. That is, when the schedule is slow, the maximum velocity increases with the initial acceleration.

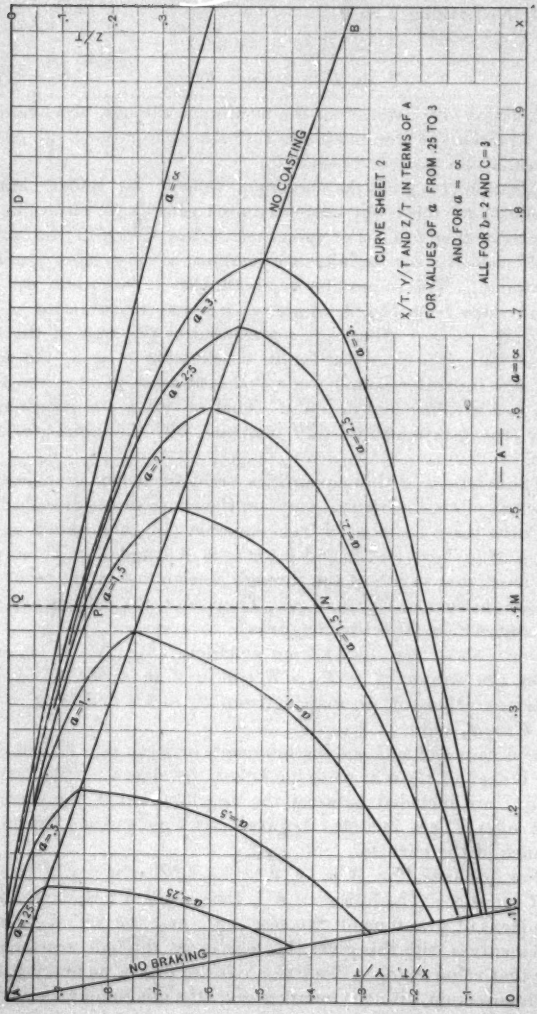
The relations of these quantities are best shown graphically; they are plotted in various ways on the curve sheets described.

Curve sheet 1 is plotted from equation (15), and shows A_0 in terms of (a) for $b = .2$, and for $c = 2$, 2.5 and 3. These are the maximum values of the through acceleration A in terms of (a) for the several values of (c). They show the influence of (c), the retardation after braking, on A_0.

Curve sheet 2 is plotted from equations (6), (7) and (8), and gives the values of x/T, y/T and z/T in terms of A for different values of (a) varying from .25 to 3.0, and for $a = \infty$; for $b = .2$, and $c = 3$.

The values of x/T are the intercepts between the X axis and the branch of the a curve lying below the line A B; the values of y/T are included between the two branches of the a curve, and of z/T are included between the line A D and the upper branch of the a-curve.

For instance, for $A = .4$, $a = 1.5$, $x/T =$ M N, $y/T =$ N P, $z/T =$ P Q. The line $a = \infty$ is the axis of X; the line A B is the locus of $y = 0$ or of "no coasting," and the intersection of the a-curves with this gives the maximum through acceleration for that value of (a). The line A C is the line of no braking, and its intersection with the a-curves gives the minimum values of A.


CURVE SHEET 2
X/T. V/T AND Z/T IN TERMS OF A
FOR VALUES OF a FROM .25 TO 3
AND FOR a = ∞
ALL FOR b= 2 AND C=3
NO COASTING
NO BRAKING
a=∞
a=3
a=2.5
a=2
a=1.5
a=1
a=.5
a=.25
Z/T
V/T
X/T
A
B
C
D
M
N
P
Q
X

These curves give all possible values of A, and enable Fig. 1 for any particular case to be drawn at once.

Curve sheet 3 is plotted from equation (9), and gives $a\,x/T$ in terms of A for different values of (a), and for $b = .2$, $c = 3$. These curves show at a glance the variation of maximum velocity with different initial acceleration, and the crossing of the a-curves.

Curve sheet 4 shows $a\,x/T$ in terms of A for $b = .2$ and for $c = 2.0$. It illustrates the influence of the braking effort on the maximum velocities.

These curve sheets give a complete determination of the kinematics of the problem. There remains the determination of energy and power—the kinetics. I limit the discussion to the case in which power is applied along O A (Fig. 1) only, and the car is allowed to "coast" with gradually diminishing velocity due to the constant retardation (b) from A to B where a braking force producing a retardation $(c - b)$ is applied, making a total retardation equal to (c).

The assumption of (b) constant is necessary to a simple discussion; if it is not so, an average value may be used—considerable variations in the value of (b) make little difference. I use throughout $b = .2$ M.P.H./sec., equal to a force 18.2 lbs. per ton; (b) represents the total tractive resistance on a level at uniform speed, and with electric motors, includes motor friction.

The force exerted along O A (Fig. 1) is

$$f = \frac{W}{g}(a + b), \text{ lbs.}$$

and the total energy input at the car axle, per ton, up to the point A is

$$2.50 \times 10^{-2}\,(1 + b/a)\,\overline{a\,x^2}\ \text{wh} \qquad (17)$$

Of this energy

$$2.5 \times 10^{-2} \times \overline{a\,x^2}\ \text{wh}$$

is represented by the kinetic energy at point A, and

$$2.5 \times 10^{-2}\,\overline{a\,x^2} \times b/a\ \text{wh}$$

is the work done in overcoming tractive resistances up to point A.

The power per ton is

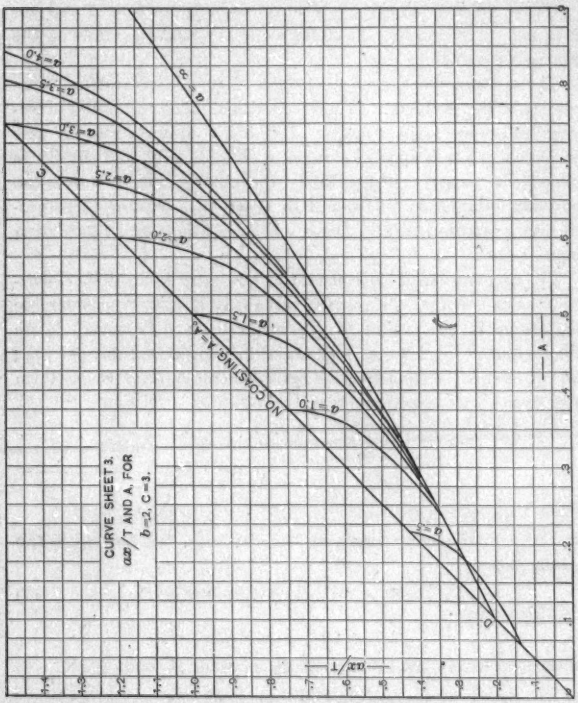
$$P = 0.182\,(a + b)\,a\,x\ \text{kw} \qquad (18)$$

Schedule Speed.—The relation of schedule speed S to average

velocity V is dependent upon the time of stops at the station. If t is the time of stop at station, then

$$T = .682\ L/S - t$$
$$V = .682\ L/T$$
$$A = V/T = 1.47\ V^2/L \qquad (4)$$

These equations give A in terms of S and L. If t is taken at

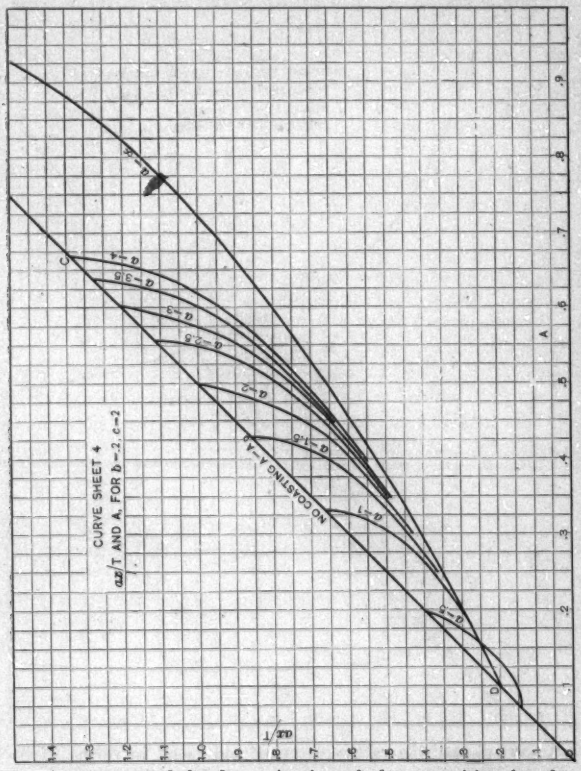


any constant value, curves can be plotted giving V in terms of S and L; it is, however, simpler to use the equations.

I do not consider the schedule speed further, but assume V and T to be determined from S, (t) and L. Knowing A, Curve sheet 2 gives x/T and hence $(a\ x)$ for any values of A. The energy and power are then determined by equations (17) and (18).

These quantities refer to the car axle and do not include losses of any kind in the motor, and hence are of theoretical interest only. If motors had constant efficiency, these values would be proportional to the energy input.

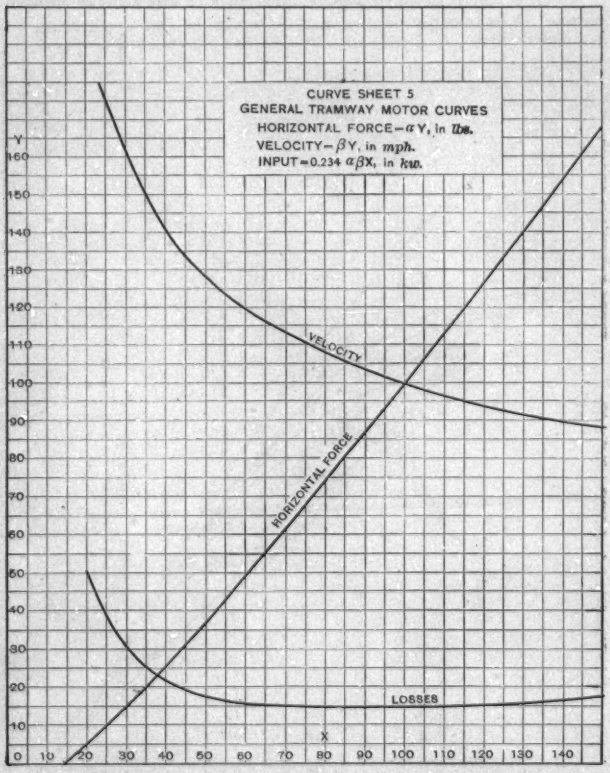
The discussion to this point is applicable to any kind of motive power. The matter of practical interest is the application to



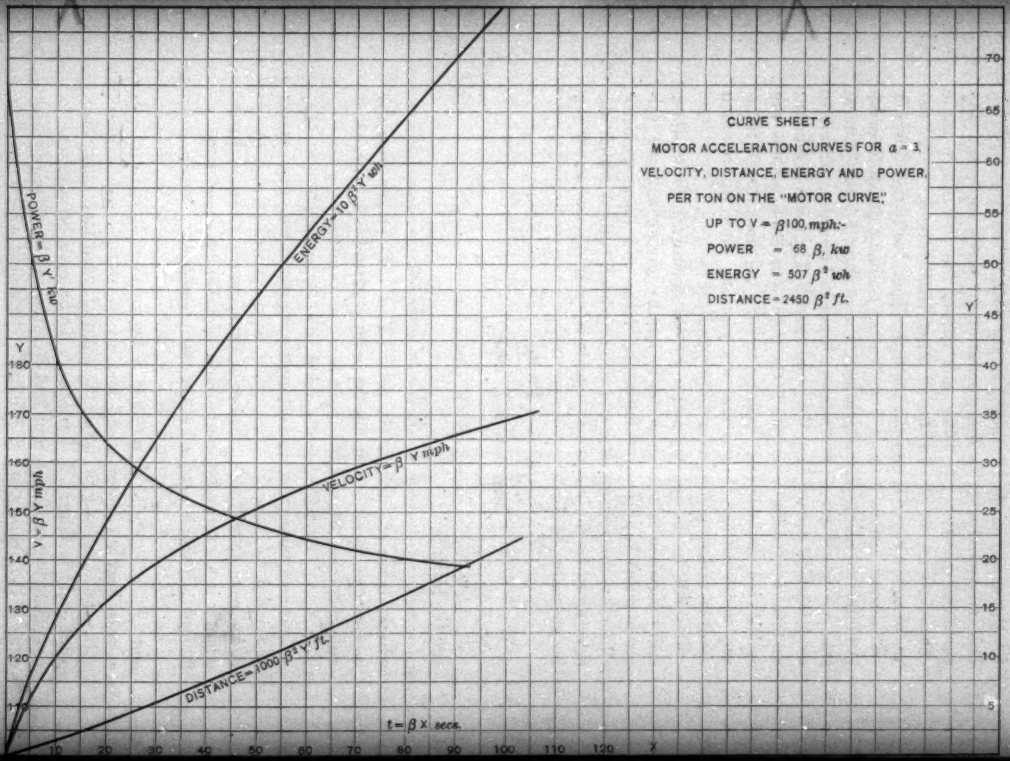
electric motors, and the determination of the quantities for the $(v\text{-}t)$ curve of Fig. 2; the characteristics of the motor must be taken into account.

General Motor Curves.—In order to make the discussion general, it is not sufficient to consider one or two sizes of motor or

one or two gearing ratios—all sizes and all gear ratios must be included, or what is the same thing, all values of torque at the axle and all values of velocity; in a word, a set of motor curves applicable to all sizes of motors must be prepared.



I have plotted the values of torque and speed in terms of input for some twenty tramway motors of different sizes and makes, expressing all quantities in percentage of their value at rated load, and find that the agreement is close—so close that one set of curves can with sufficient exactness for this purpose be used


CURVE SHEET 6
MOTOR ACCELERATION CURVES FOR $\alpha = 3$.
VELOCITY, DISTANCE, ENERGY AND POWER.
PER TON ON THE "MOTOR CURVE."
UP TO V = β100, mph:-
POWER = 68 β, kw
ENERGY = 507 β^2 wh
DISTANCE = 2450 β^2 ft.
POWER = β Y kw
ENERGY = 10 β^2 Y wh
VELOCITY = β Y mph
DISTANCE = 1000 β^2 Y ft.
V = β X mph
t = β X secs.
Y
X
10 20 30 40 50 60 70 80 90 100 110 120
170 160 150 140 130 120 110
70 65 60 55 50 45 40 35 30 25 20 15 10 5

for almost any modern tramway motor built in the United States. It is immaterial whether this general motor curve represents any particular motor. By using this curve, the identically same characteristics are retained for all initial accelerations, hence the use of such a curve brings out the differences due to the different initial accelerations without complicating them by the differences in individual motor characteristics. Curve sheet 5 is this general tramway motor curve; abscissæ are input in kilowatts; ordinates are horizontal force (f) in lbs., and speed (v) in miles per hour; on these curves the rated load is the point (100,100) and is the rating on a one-hour basis, heating to 75° C.—the common tramway motor rating. Losses also are plotted; they differ much more in various motors than do (f) and (v).

Let $\beta Y'$ be the velocity and $\alpha Y''$ the horizontal force, then every variation in conditions can be met by giving suitable values to α and β; this use of horizontal force and linear velocity eliminates the gearing ratio and size of wheel, both of which are determined by considerations outside of this discussion. Then for any value of X,

$$f = \alpha Y'' \text{ lbs.}$$

$$\text{and } v = \beta Y' \text{ mph.}$$

The output is

$$[\alpha \beta Y' Y'']/503 \text{ kw.}$$

and the input is

$$[\alpha \beta Y' Y'']/(e \times 503) \text{ kw.}$$

where e is the efficiency at the point (X, Y', Y'') taken from the motor curve sheet. Choosing the point (100,100,100), the rated load, then $e = .85$, and the input is

$$P = 0.234 \alpha \beta X, \text{ kw.} \qquad (19)$$

This determines the constant multiplier for the input in terms of α and β.

An example will make the use of these curves clear; suppose a motor is to exert a horizontal force of 2,000 lbs. at a speed of 20 mph.; if this is required at the rated load then

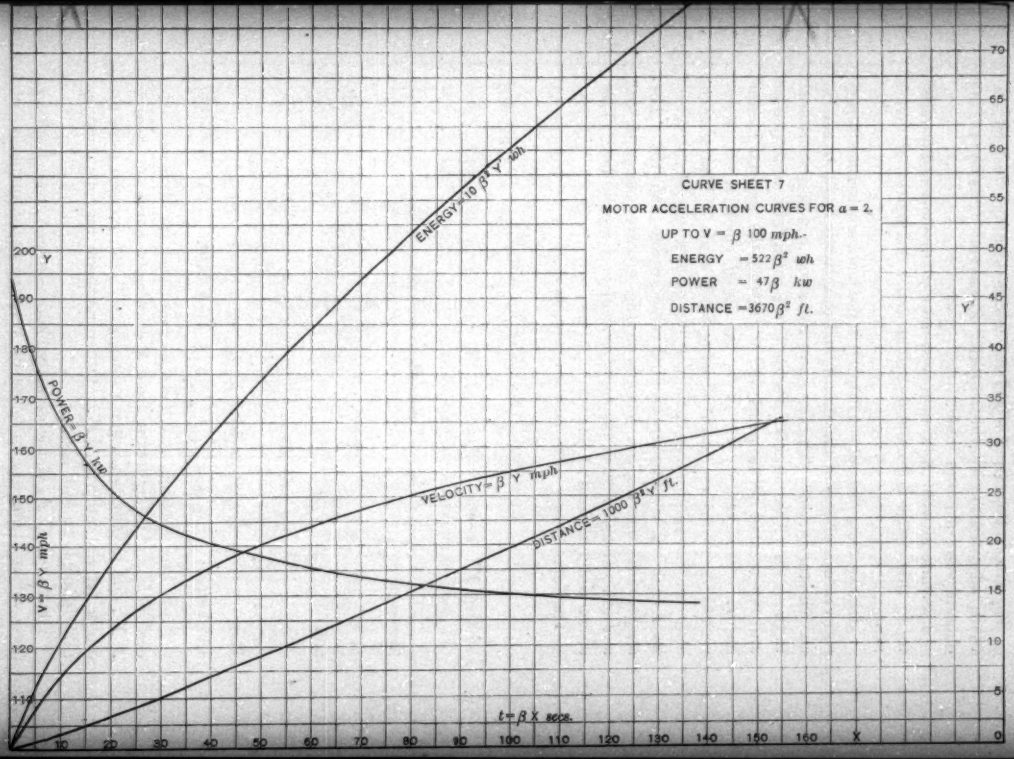
$$\alpha = 20, \quad \beta = .2$$

and

$$P = 0.936 X \text{ kw.}$$

at rated load this motor is 93.6 kw.

It is not necessary to choose the point of rated load, any other point could be taken; for instance, a motor may be assumed to give 2,000 lbs. at 125% torque and 30 mph. at the corresponding value of the velocity, which is 93.8%; the motor is then carrying a load of 119%.


CURVE SHEET 7
MOTOR ACCELERATION CURVES FOR $a = 2$.
UP TO V = β 100 mph.-
ENERGY = $522\beta^2$ wh
POWER = 47β kw
DISTANCE = $3670\beta^2$ ft.
ENERGY = $10\beta^4 Y'$ wh
POWER = βY kw
VELOCITY = $\beta Y'$ mph
DISTANCE = $1000\beta^4 Y'$ ft.
$t = \beta X$ secs.
V = β mph
Y
Y'
X

The gear ratio, size of wheel and motor capacity must be mutually adjusted to give the required force and velocity at the chosen point of motor input.

I assume in what follows that α and β are chosen for rated load; this matter is discussed further under the matter of motor rating.

Acceleration Curves.—M (Fig. 2) is the point where the external resistance is all out of the circuit, and the motors begin to run on the "motor curve." The force exerted along O M is proportional to $(a + b)$. Assume this force to be the rated force of the motor and represented by the point $Y'' = 100$ on the general motor curve. The velocity at M is the rated velocity, is represented by $Y' = 100$, and is equal to β 100 mph. The acceleration on the motor curve continues to a point N where the velocity is $\beta\ Y'$, Y' having any desired value greater than 100. The shape of the curve M Q N depends on the relative values of (a) and (b). If a/b is large the curve is steep; if small, flat. This curve is determined as follows:

The force at any point on the curve, as Q, is

$$\text{Force} = \frac{W}{g}[d\ v/dt + b] = a\ Y'' \tag{20}$$

where Y'' is the ordinate of the (f) curve of sheet 5. For $Y'' = 100$ at rated load and point M,

$$d\ v/dt = a$$

hence

$$a = \frac{W}{g} \times \frac{(a + b)}{100} \tag{21}$$

and

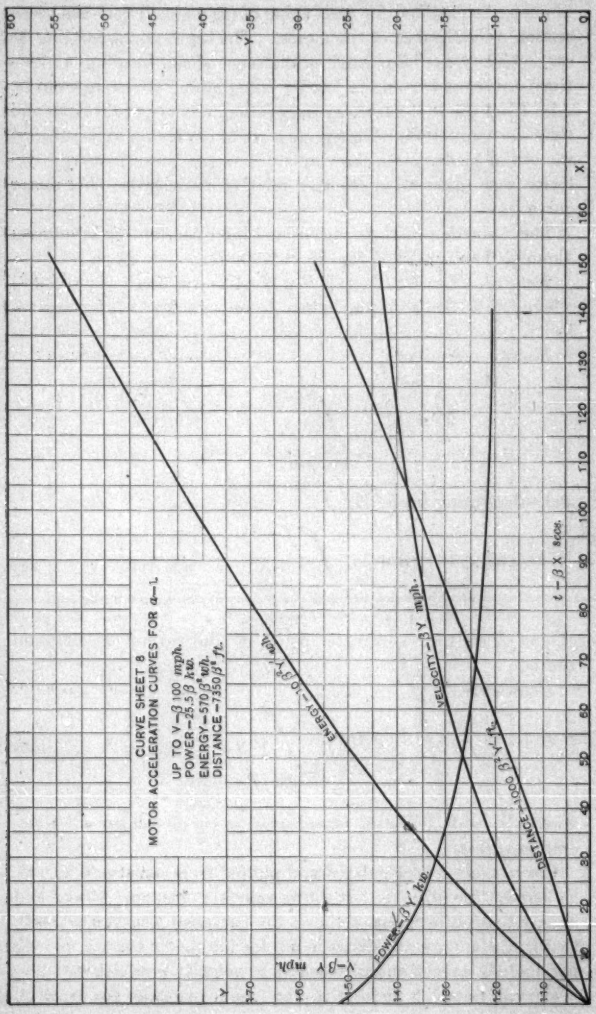
$$d\ v/dt = \frac{(a + b)}{100}\ Y'' - b$$

But $$d\ v/dt = \beta\ [d\ Y'/dt] = \beta\ [\triangle\ Y'/\triangle t]$$

and hence $$\triangle\ t/\beta = \triangle\ Y'/\ [(a + b)\ Y''/100 - b] \tag{22}$$

From this equation, $\triangle\ t/\beta$ can be calculated for different values of (a) and (b) by taking from the velocity curve of sheet 5 $\Delta\ Y'$, and the corresponding values of Y'', the average force ordinate for the interval, $\Delta\ Y'$. Other methods may be used to calculate this tangent curve.

The values of $\Delta\ t/\beta$ are calculated and summed up. Plotting these values as abscissæ in terms of velocity as ordinates, a set


CURVE SHEET 8
MOTOR ACCELERATION CURVES FOR a—L
UP TO V—β 100 mph.
POWER—25.5 β³ kw.
ENERGY—570 β² wh.
DISTANCE—7350 β² ft.
V—β Y mph.
ENERGY—10 β² Y² wh.
VELOCITY—β Y mph.
POWER—β³ Y³ kw.
DISTANCE—100 β² Y² ft.
t—β X secs.
Y
X

of curves is obtained for each value of (a) which I call the acceleration or (v-t) curve of the motor. The input corresponding to each velocity can be plotted on the same sheet, forming a (kw.-time) curve, and from the last by integration, an energy curve. Also from the (v-t) curve, by integration, a curve of distance and time can be plotted. This gives a set of curves for each value of the initial acceleration, showing at once the power, energy, velocity and distance up to any relative velocity on the motor curve.

The deduction of these curves and their constants is as follows: From (19) and (21) and for $W = 2{,}000$ lbs.

Power.—$P = 0.213\ (a + b)\ \beta \times X$ kw./ton (23)

where X is the abscissa, taken from curve sheet 5 for any velocity.

Energy.—Similarly,

$$W = \frac{1000}{3600} \int_{v-\beta 100}^{v-\beta Y} P\, dt \qquad \text{wh./ton}$$

and substituting from (23)

$$W = 5.92 \times 10^{-2}\ (a + b) \int_{100}^{Y} Y\, dX \qquad \text{wh./ton} \qquad 24)$$

Y and X are the co-ordinates of P on the several acceleration sheets.

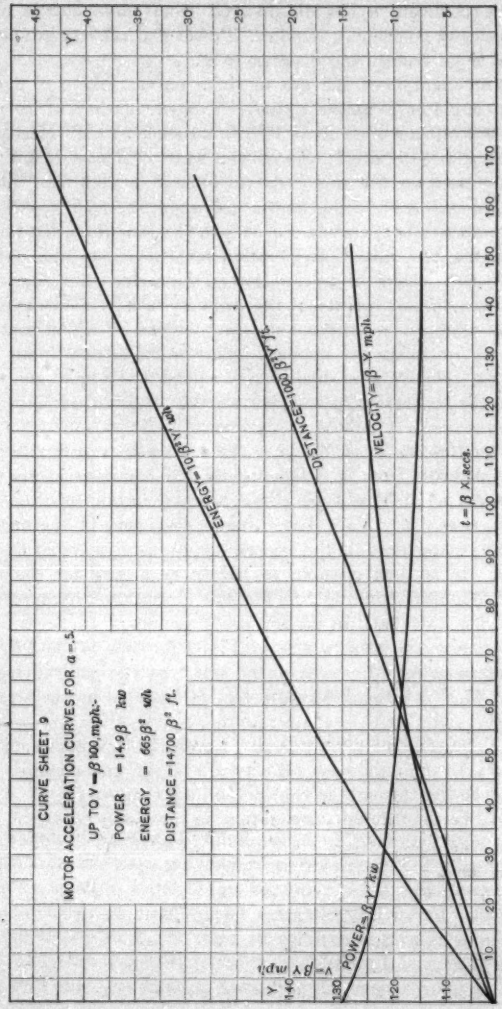
Distance.—

$$S = 1.467 \int v\, dt = 1.467\ \beta^2 \int_{100}^{Y} Y\, dX \qquad (25)$$

where Y and X are the co-ordinates of (v) on the several acceleration sheets.

Curve sheet 6 is calculated and plotted from equations 22, 23, 24 and 25, for (a) = 3. It shows velocity, distance, power and energy per ton in terms of time; the curves all start at $v = \beta 100$, that is, the energy and distance are for the motor curves, and do not include energy and distance up to velocity $v = \beta\ 100$.

Curve sheets 7, 8 and 9 give the same quantities for (a) = 2, 1, and 0.5, respectively.


CURVE SHEET 9
MOTOR ACCELERATION CURVES FOR $a = .5$
UP TO $V = \beta$ 100 mph:-
POWER $= 14.9\beta$ kw
ENERGY $= 665\beta^2$ wh
DISTANCE $= 14700\beta^2$ ft.
ENERGY $= 665\beta^2$ wh
DISTANCE $= 100\beta^2$ ft.
VELOCITY $= \beta$ V mph
POWER $= \beta$ V kw
$t = \beta$ X secs.
V mph
Y
0
5
10
15
20
25
30
35
40
45
10
20
30
40
50
60
70
80
90
100
110
120
130
140
150
160
170
180

The coefficient a has disappeared from these curves, only β the velocity coefficient, remains. Power and velocity are proportional to β; energy and distance to β^2.

As an example of the use of these curves, take $a = 3$, curve sheet 6. For a motor to have a rated velocity of 20 mph., $\beta = 0.2$; after a time $X = 70\,\beta = 14$ seconds, the velocity will be $159\,\beta = 31.8$ mph.; the power $21.0 \times \beta = 4.20$ kw.; the energy used on the motor curve,—$590\,\beta^2 = 23.6$ wh., and the distance traversed on the motor curve, $14{,}250 \times \beta^2 = 570$ feet.

From these (v-t) curves, Fig. 4 is drawn, on which the line O M represents the initial acceleration continued to the velocity $v = \beta\,100$, where the acceleration on the motor curve begins and continues to any point N; the velocity R N is made anything within practical conditions; coasting begins at N and continues to B, where brakes are applied. The ordinates multiplied by β give the velocity, the abscissæ multiplied by β give the time; hence the inclination of any line on this diagram is independent of β.

Correction to Type Curve:—This discussion and the curve sheets deduced from it, determine all the elements of the type curve, O A B C. This type curve bears a certain relation to the motor curve O M N B C. The distance traversed in the first case will be greater than in the second by the equivalent of the area M A N. It is then possible to reduce each case for the motor curve to a corrrsponding case for the type curve, by applying a correction, calculated in the following manner:

Determine the area M A N of Fig. 4 for each (a), and for any desired number of points on the motor curve; call this area Δ; then $1.47\,\beta^2\,\Delta$ is the distance in feet represented by the area.

Let

L'_t be the maximum velocity A D on the type curve:

L'_m be the maximum velocity R N on the motor curve;

A be the through acceleration on the type curve;

A_m be the through acceleration on the motor curve;

and

X be $\overline{\text{O C}}$

Then

$$L = L' - 1.47\,\beta^2\,\Delta$$

$$\beta^2\,A_m\,X^2 = \beta^2\,A\,X^2 - \beta^2\,\Delta$$

or

$$A_m = A - \Delta/X^2$$

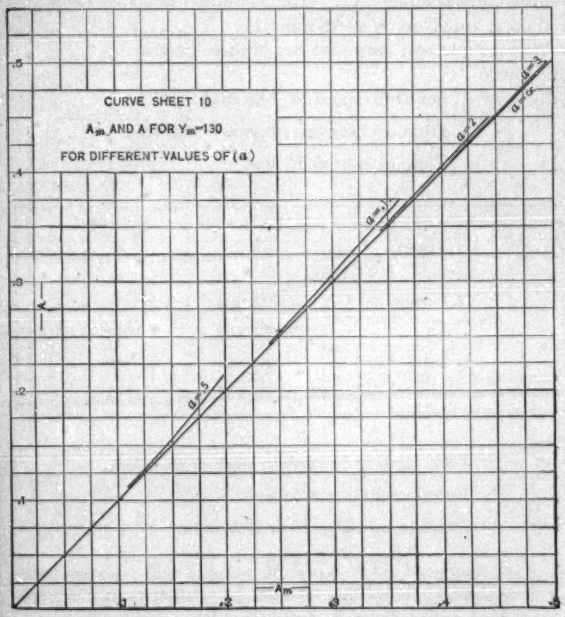
Put

$$\lambda = a\,x/T = Y_t/X \tag{26}$$

then

$$A_m = A - \Delta\,(\lambda/Y_t)^2 \tag{27}$$

Y_t is found from curve sheets 6, et seq., and λ from curve sheet 2, for any values of A, (a) and Y_m; A_m is then calculated, and plotted in terms of A, using the values of Δ previously calculated.



The values of A_m for all values of A, Y_m and (a), have been determined in this way; the corrections for values of Y_m less than 130 are too small to be taken into account; as I shall show, the minimum energy required for all schedules and for all values of (a), is practically at $Y_m = 130$; in some cases greater values of Y_m give slightly less energy, but the difference is so trifling that I have used only this value in discussing the energy relations.

Curve sheet 10 shows A_m in terms of A, for $Y_m = 130$, and for the different values of (a).

Maximum A_m:—The maximum A_m is evidently attained when acceleration is continued to the braking point and is represented by O M N C′ (Fig. 4). To calculate this maximum, O R is found from curve sheet 6 et seq., R C′ from the known values of R N and (c);—then R C′/O C′ $= z/T$.

Curve sheet 2 gives at once the values of A, and curve sheet 10, the corresponding A_m. The values so calculated for $Y_m = 130$ only, are given on curve sheet 11.

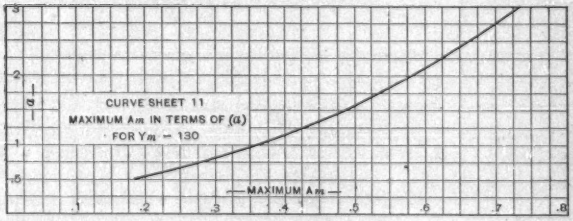
An example best shows the use of these curves:

Suppose

Schedule speed = 16.5 mph.

Distance between stops = 2,000 feet.

Time of stop = 15 secs.



Then,

Total time from start to start = 82.4 secs.

Running time = 67.4 secs.

Average velocity, $V = 20.2$ mph.

$$A = V/T = .3$$

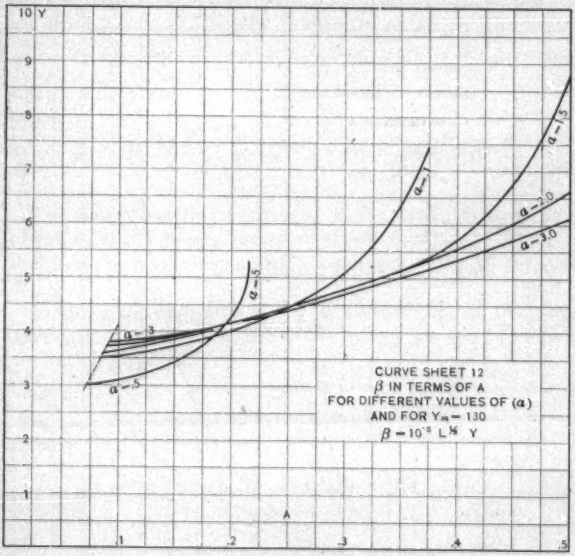
Curve sheet 11 shows that (a) must be greater than .75; take $a = 1$; then curve sheet 8 determines the velocity curve up to the point N (Fig. 4); for $Y_m = 130$, curve sheet 10 gives $A = .305$ and from curve sheet 2,

$$x/T = .477$$
$$y/T = .130$$
$$z/T = .393$$

Maximum velocity on type curve = .477 × 67.4 = 32.3 mph.

Maximum velocity on motor curve $= 32.2 \times 130/136 = 30.7$ mph.
Velocity at braking $= 3 \times .130 \times 67.4 = 26.3$ mph.

Energy Input.—The energy at the car axle has already been determined; the electric input depends upon the method of motor control and the efficiency of the motor. I assume that series-parallel control in two steps is used. The power at the axle at



rated velocity, that is, point M, is then $0.85\ P_0$, where P_0 is the rated power, and the power at the axle is proportional to the speed. The change from series to parallel is assumed to occur at half speed; the efficiency of the motors will be about 70% when in series and carrying rated current. With these figures the input up to $v = \beta\ 100$ is

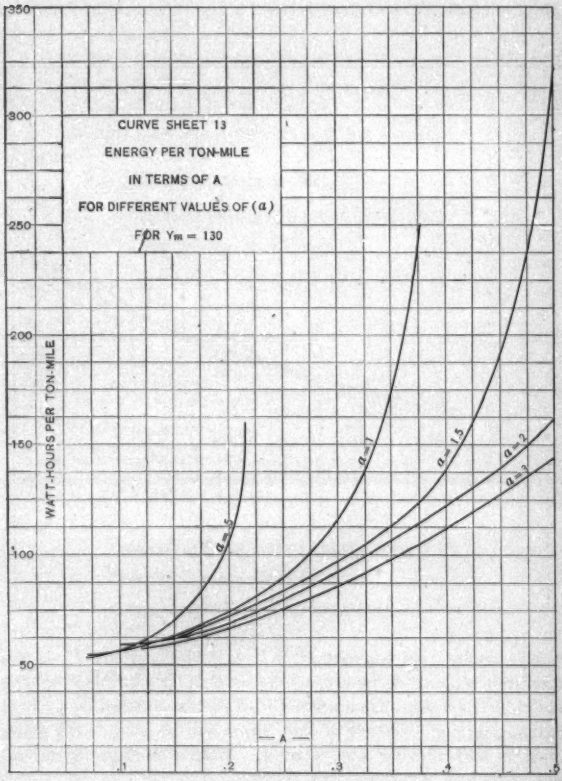
$$W_0 = .8\ P_0\ t_1, \tag{28}$$

and the output is

$$W' = .425\ P_0\ t_1,$$

hence the efficiency up to rated velocity—while resistance is in circuit—is 53%.

Substituting for P_0 its value from equation (23), and $t_1 = 100/\beta$, the input of electric energy per ton up to the point M is



$$W_0 = 475\,(1 + b/a)\,\beta^2 \qquad (29)$$

The values calculated from this equation are included on curve sheets 6, 7, 8 and 9, giving the energy up to the point $v = \beta 100$.

The input from the point M to the point N on the motor curve, is taken directly from curve sheets 6 et seq. depending upon the initial acceleration.

Velocity Constant, β.—Energy, power and distance all involve the velocity constant β. The values of β can be expressed in terms of the distance traversed, for all values of the through acceleration, as follows:

$$.682\ L = A\ T^2$$

$$T = \beta\ X$$

and

$$X = Y_t/\lambda;$$

hence

$$\beta = \left(\frac{.682\ L}{A}\right)^{\frac{1}{2}} \times (\lambda/Y_t) = \left(\frac{.682\ L}{A}\right)^{\frac{1}{2}} \frac{a}{Y_t}\frac{x}{T} \qquad (30)$$

The energy used is proportional to the product of β^2 by the ordinates of the energy curves on sheets 6 et seq. There will, therefore, be a different multiplier for each value of (a) and of Y_m.

Limiting the discussion to $Y_m = 100$ and $Y_m = 130$, the following table gives the values of the energy per ton in terms of β.

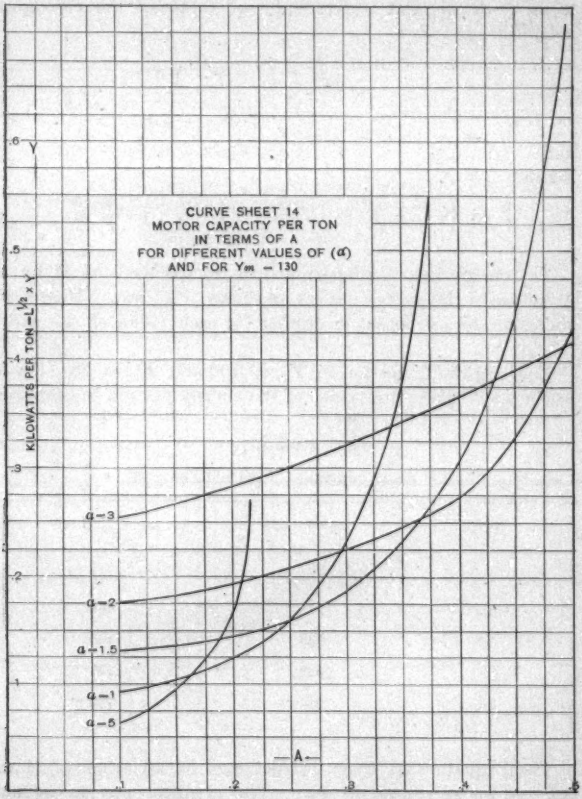
TABLE I.

Energy up to $v = \beta\ 100$ and $v = \beta\ 130$

(a)	$v = \beta\ 100$	$v = \beta\ 130$
.5	665 β^2	1115 β^2
1.	570 "	865 "
1.5	535 "	800 "
2.	532 "	767 "
3.	507 "	733 "

From equation 30, β can be calculated for all values of A and (a), and for all values of Y_m. The simplest way to calculate these values is, first to assume values of A and (a); then since $Y_m = 130$, Y_t is fixed; the value of x/T is taken from curve sheet 2.

β is proportional to $L^{1/2}$; β^2 to L; hence the energy used is proportional to the distance for any given value of A. The values of β have been calculated from this equation for all values of Y_m; I give here, however, a curve sheet showing these values



only for $Y_m = 130$, for the reason explained below.

This sheet covers the entire range of values of β for $Y_m = 130$, from the minimum to $A = .5$; this value of A is greater than is possible in practice.

The energy for all schedules can now be calculated by multiplying β^2 from curve sheet 12 by the constants given in table 1. The values so calculated for $Y_m = 130$ are plotted on curve sheet 13.

I have calculated the energy consumption in this manner for all values of Y_m and for the different values of (a) and of A, up to $Y_m = 160$. To give all these results is of no value, since the result shows that in nearly all practical cases the energy input is substantially a minimum for $Y_m = 130$. Table 2 below shows the relative energy for different values of Y_m for $(a) = 2$, in terms of energy for $Y_m = 130$, as 100%, for the various values of A.

TABLE 2.

Relative Energy for Different Use of Motor Curve for $(a) = 2$. in Terms of Energy for $Y_m = 130$ as 100%.

Y_m	A				
	.5	.4	.3	.2	.1
160	——	102%	97%	91%	89%
150	104%	101.5	98	93	93
140	100	100	99	95.5	95
130	100	100	100	100	100
120	103.5	106.5	106.5	105.5	105
110	110	112.5	113	112	108
100	119	122	121	128	120

This table is a fair example; it shows that the difference between the energy for 130 and greater values of Y_m is comparatively small. In the rest of this discussion, I use this value of Y_m only, and the values of β are for this reason given only for $Y_m = 130$.

Curve sheet 13 gives at once the answer to all questions regarding the energy required for any schedule; it shows clearly how slight is the saving in energy effected by the use of rapid accelerations. For instance, for $A = .25$, the energy for an acceleration of one mile per hour per second is 17.2 wh.; for an acceleration of 3 mph./sec, it is 14.5 wh.; that is, increasing the

acceleration in the ratio of three to one diminishes the energy required only 16%.

Motor Capacity:—This discussion has been based throughout on the assumption that the motor operates at the rated capacity on the one-hour basis, when at the point *M* (Fig. 4). On this assumption, the following table gives the capacity of the equipment required.

TABLE 3.

(*a*)	$P_0 - \beta \times$
.5	14.9 kw/ton
1.	25.5 "
1.5	36.3 "
2.	47. "
3.	68. "

These are the starting values of the power curves on sheets 6, 7, 8 and 9, and may be considered the rated capacity of motors required, in so far as this rating is determined by commutation. Taking into account grades and curves and the possibility of various emergency requirements, I think it conservative to say that a motor should not be designed to operate above its one-hour capacity under the conditions here assumed, that is, on straight, level track, with frequent stops. The capacity of the motor depends also upon heating, and in most cases this is the ruling consideration. Hence it becomes necessary to determine the amount of heat liberated in the motor under all conditions of initial and through acceleration.

Heat Losses:—Referring to Fig. 4, the heat losses in the copper up to the point *M* are directly proportional to the time, since heat is liberated at a constant rate. The magnetic flux density in the motor is constant up to this point; if the core loss were proportional to the speed, the average loss would be one-half of the loss at rated speed; on the other hand, if the core loss were proportional to the square of the speed, the average loss would be one-third of the loss at rated speed; hence the average core loss

CURVE SHEET 15
RATIO OF WEIGHTS OF EQUIPMENT (p)
IN TERMS OF KILOWATTS OF MOTOR CAPACITY
PER TON OF TOTAL WEIGHT
WEIGHT RATIO (p)
KW. PER TON
3.0
2.8
2.6
2.4
2.2
2.0
1.8
1.6
1.4
1.2
1.0
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15

up to the point o M is between 33 and 50 per cent. of this loss at rated speed. The actual variation of the core loss with speed lies between these two values; I assume it to be equal to 41.0% of the loss at rated speed. It is of comparatively little consequence whether it be taken at 33 or 50 per cent. or at any intermediate value, since the core loss at rated speed is less than 30 per cent. of the total heat loss at rated speed; considerable errors in its values are of little moment.

I find from an examination of a number of modern tramway motors that at rated output P_0 the average core loss is 3.4%, and the average copper loss 8.6%,—a total for the average loss of 12%, excluding friction losses. These losses are the average of a somewhat different group of motors from those used in preparing the general motor curve, hence the small loss left for friction; for this last group the efficiency is 83% instead of 85%. The variations of the separate losses in the different motors is, generally speaking, small, although in certain motors the disparity is greater. The average loss up to $v = \beta\ 100$ is then 10% of the rated capacity, and is independent of initial acceleration.

From M to N on the motor curve, I have determined the losses from curves of separate losses; for the same group of motors the average of the total heat loss from $v = \beta\ 100$ to $v = \beta\ 130$ is 8.5% of the rated capacity.

Up to $v = \beta\ 100$, the average loss is 10% of P_0; for $v = \beta\ 130$, the values of t_1 and t_2 are taken from sheets 6, 7, 8 and 9, and the average loss over the total time $(t_1 + t_2)$ calculated; it will be found to be constant for all values of (a), as it should be, and equal to 9.4% of rated capacity.

The average rate of heat dissipation will be:

$$.1\ P_0, \quad \text{up to } v = \beta\ 100$$
$$\text{and } .094\ P'_0, \quad \text{up to } v = \beta\ 130.$$

The rates continue for the times t_1 and $(t_1 + t_2)$, respectively; during the rest of the cycle the motors are radiating heat; hence these rates referred to the total time T, are

$$.1\ t_1/T \times P_0, \quad \text{up to } v = \beta\ 100,$$
$$.094\ (t_1 + t_2)/T \times P'_0, \quad \text{up to } v = \beta\ 130,$$

and as percentages of the rated capacities in the two cases, are

$$10\ t_1/T\% \qquad \text{up to } v = \beta\ 100. \qquad (31)$$
$$9.4 \times (t_1 + t_2)/T\% \qquad \text{up to } v = \beta\ 130. \qquad (32)$$

These are the average rates for the entire time from start to stop, and by comparison with the average loss that such a motor will stand continuously without excessive heating, determine whether the motors would be running above or below their rated capacity; or conversely, determine what the capacity on a one-hour basis must be, so that when operating in this way they will not overheat.

Equation (32) can be written

$$9.4\ (1 + t_2/t_1)\ t_1/T\%$$

But (Fig. 4)

$$t_1 = \overline{OP} = OD \cdot MP/AD$$

$$= x \times 100/Y_t$$

$$\text{and } t_1/T = x/T\ (100/Y_t)$$

hence the total heat loss up to the velocity β 130 averaged over the entire time T is

$$9.4\ (1 + t_2/t_1) \times 100/Y_t \times x/T\% \qquad (33)$$

where x/T is taken from curve sheet 2 for the proper values of A and (a). In this deduction it is assumed that $A_m = A$.

Hour Rating.—The one-hour rating of a tramway motor is much in excess of the continuous capacity; approximately such a motor will carry its rated output for 25% of the total time, that is, for one minute out of four; the heat generated at rated capacity for one-quarter of a complete cycle will bring it to its rated temperature. It is assumed that the cycles are repeated at such intervals that a permanent regime is attained. This is only an approximation, but is a fair one, if the percentage be more or less than 25, and the conclusions can be altered to accord.

As the average heat losses at rated load are 12%, it follows that such a motor can dissipate continuously 3% of its rated capacity; when the average rate is over 3% the motor is overloaded.

Since the average loss that a motor will stand is equal to 3%, it follows that the maximum value of (x/T), or $(x/T)_0$ is

$$(x/T)_0 = 3/[9.4\ (1 + t_2/t_1)] \times Y_t/100$$

Curve sheets 6, 7, 8 and 9 give then

TABLE 4.

(a)	$(x/T)_0$
.5	0.249
1.	0.260
1.5	0.265
2.	0.270
3.	0.271

For $Y_t = 100$, the limiting value is $(x/T)_0 = .3$ for all values of A and (a).

This means that for schedules in which $x/T > .3$, in one case, and for $x/T > .249$ et seq. in the other, the capacity must be greater than the capacity determined by commutation, that is, greater than the values of Table 3 in the ratio of $(x/T)/(x/T)_0$; for $x/T < .3$, or less than the values of Table 4, commutation determines the capacity.

The motor capacity necessary both to avoid overheating and to keep within commutation limits, can now be calculated from these data. Table 3 gives the capacity required on the assumption that commutation is the only limit to output, and on the further assumption that the hourly rated capacity is the practical limit determined by commutation. These values multiplied by the ratio $(x/T)/(x/T)_0$ give the capacity in all cases, with the limitation that the capacity shall not be less than that of Table 3. Then,

$$\text{Motor capacity} = 14.9\,\beta \times (x/T)/(x/T)_0 \quad \text{for } a = .5 \quad (34)$$

and, $$\text{Motor capacity} > 14.9\,\beta \quad \text{for } a = .5$$

The coefficient for other values of the initial acceleration is given by Table 4.

The capacity of the motor, as determined by this equation, has been calculated for all values of A and (a), and curve sheet 14 gives the kilowatts per ton, considering both commutation and heating, for $Y_m = 130$.

For the lowest acceleration, $(a) = .5$, heating is the limitation for all values of A; for the higher values of (a), commutation is the limitation for the low, and heating for the high, values of A.

I wish to emphasize the conditions under which these capacity

curves were obtained; (1) that the limit of input determined by commutation is the rated capacity, and (2) that the limit determined by heating is an input that will average for the total time 3% of rated capacity in copper and core losses. I am aware that motors are operated at much greater inputs than the rated capacity in regular service, but for service requiring frequent stops this is not unduly low, if low maintenance cost is taken into account. It is easy to follow out this argument with any desired overload at starting; the results will be but slightly affected.

Similarly, with reference to heating, 3% is not a sacred figure, but only a fair average; it is easy to make the changes for any other value. No changes within practical limitations will affect the relative results for different schedules and initial accelerations; the absolute values only will be slightly changed.

As this entire discussion has been based on the assumption that the input of the motor along the line o m of Fig. 4 (that is, during the period of constant acceleration) is its rated capacity, it would seem at first glance, that the increase of capacity necessitated by heating conditions, would vitiate all the deductions given here. This, however, is not the case. The velocities are in no way affected; hence the energy at the car axle is unaltered. The only change brought about by using a motor that does not operate at its rated capacity during the initial period, is a change in the heat losses in the motor. This will not affect the energy input greatly, since almost the entire losses during this period are losses in external resistance; hence the curves showing energy required per ton mile will be substantially unaltered. Such change will in all cases increase the heat waste in the motor, hence the capacity, given on curve sheet 14, will in all cases be somewhat too small. On the other hand, the capacity so calculated is greater than necessary, as no allowance has been made for heat radiation during the time of stop at stations.

Another consideration leads to the same result. As I have shown, the energy used per ton mile varies very slightly for any amount of acceleration on the motor curve from 120% to 160%. To use a motor at other than the rated capacity means that the motor is operating on a different part of the motor curve; hence the difference in energy input will be small.

Weight of Equipment:—These curve sheets show that the motor capacity for high initial accelerations is generally greatly in excess of the motor capacity required for low initial accele-

rations. Greater capacity of equipment necessarily means greater weight of equipment, not only in the motors themselves, but also in the trucks, controllers, and other parts. This entails the further necessity of more energy to carry this greater weight; each addition of weight means a further addition to the energy necessary. To get a correct comparison of the total energy per mile required for different initial accelerations, the values of curve sheet 13 should be multiplied by the ratio of the necessary weights in the two cases. For instance, for an initial acceleration of 3 mph./sec, the total weight of equipment may be 25% greater than for an initial acceleration of 1 mph./sec; the total energy used per mile will then be increased in the ratio of increase of the weights.

I have taken the weights of all parts of a car equipment in detail for different capacity of motors, varying from 30 to 150 rated H.P., for both two and four motor equipments. These weights include motors, controllers, trolley poles, car wiring, motor trucks, trail trucks, and car body. I have deduced from these weights, curve sheet 15, on which the ordinates show the ratio of weights for different capacity of equipment, in terms of kw./ton of total weight as abscissæ. For instance, for an equipment of 4 kw./ton of total weight, the ratio is 1.44; for an equipment of 8 kw./ton, the ratio is 1.93; the increase of equipment from 4 to 8 kw./ton then increases the total weight in the ratio of $1.93/1.44 = 1.34$; that is, doubling the equipment means an increase of 34% in total weight; hence 34% in total energy used. The ordinates of curve sheet 13 giving energy per ton mile, multiplied by the ratios from curve sheet 15 for the different capacity of equipments, determined by curve sheet 14, will be proportional to the total energy required per mile.

The capacity of the equipment is proportional to $L^{\frac{1}{2}}$ and as the distance will vary in each case, it is not possible to deduce any general results; each case must be considered by itself, in this particular. I have made these calculations for a number of cases to correspond with practical conditions; the result is always that the energy used per mile is less for the lower accelerations. As an example,

Let

$$V = 16.5 \text{ mph.}$$

$$L = 2000 \text{ feet;}$$

then

$$A = .2$$

From the various curve sheets, the following table is deduced:

TABLE 5.

a	kw /ton	wh./ton	ρ	$\rho \times$ wh./ton	
				wh.	%
.5	7.6	106	1.87	198	174
1.	5.6	75	1.68	114	100
1.5	6.5	73	1.73	126	111
2.0	8.7	70	2.03	142	124
3.0	12.6	67	2.65	177	156

The last column of this table shows the energy required per mile to be the minimum for an acceleration of 1 mph./sec.; the motor capacity is also at a minimum for this acceleration.

To take the case of the proposed express service of the New York Rapid Transit Line: Here the schedule speed will be 35 mph.; the average distance between stations, about 7,500 feet; time of stop at stations, 15 seconds.

From these data,

$$V = 38.8 \text{ mph.}$$

and $$A = .275$$

The following table is deduced from these data:

TABLE 6.

a	kw./ton.	wh./ton
1.	16.7	102
1.5	14.9	82
2.	18.8	72
3.	26.9	62

This table shows the practical impossibility of accomplishing such a schedule on the assumptions of this discussion. The

minimum motor capacity required of 14.9 kw./ton, is beyond the range of practical conditions. The weight of motors, controllers, and wiring, that is, the electrical equipment alone, will be about 75 pounds per kilowatt, and the weight for 14.9 kw. would be about 1,140 pounds. This does not leave sufficient weight for the other parts of the equipment. For an acceleration of 3 mph./sec, the weight of the motors required, 26.9 kw., would be approximately one ton; that is to say, at this initial acceleration, the motors would just be able to propel themselves.

Conclusions.—My conclusion from this discussion is that the acceleration that gives the lowest motor capacity per ton is generally the most economical. The very small saving in energy is not to be compared with the many disadvantages of very rapid initial accelerations. Assume, for instance, a gain of 10 watt-hours per ton mile for an acceleration of 3, over that required for an acceleration of one; for a 20-ton car, and with energy at .5 cents per kw. hour, this represents a saving of .1 cent per car-mile. This is too trifling to be considered in comparison with the fixed charges on the greater investment for motors and distribution system; the poorer load factor at the power station, the increased cost of maintenance, the difficulty of accurate handling of cars, and, above all, the much greater discomfort to passengers.

Although I have had to make in the course of this discussion, various more or less arbitrary assumptions, I believe that they are all fair ones, representing average conditions. It is open to any one to follow this method on such other assumptions as may suit his fancy. I believe, however, that no practical assumptions will change the general results.

In discussion of Dr Hutchinson's Paper, January 24th, 1902.

A CONSIDERATION OF THE INERTIA OF THE ROTATING PARTS OF A TRAIN.

BY NORMAN WILSON STORER.

The problem of calculating the motor capacity and the amount of power necessary to maintain a certain train service involves a consideration of where all the power which is developed by the motors is expended; of just how much is used in overcoming train resistance; how much in overcoming the force of gravity, and how much in overcoming the inertia. The train resistance is a variable quantity depending on the track, bearings, wind and speed. A considerable number of formulæ have been produced to assist in calculating the train resistance under different speeds, but at the best the amount to allow for train resistance is only approximate. The energy required to overcome the force of gravity and the inertia of the train, however, is susceptible of the most exact calculation, but the latter part is seldom estimated correctly. There is one element in the inertia factor which has been almost entirely neglected, either because it has not been recognized at all or because its importance has not been appreciated. This feature is the inertia of the rotating parts of the train.

It has been generally understood that armatures of small diameter and light weight are desirable because their small flywheel capacity makes easier braking, but it has seldom been considered that this means also less power developed by the motor. A recent investigation of this subject has led to some very interesting results. It is found that the wheels with their low speed and the armatures, which usually revolve at a much higher rate than the wheels, together constitute an important element in

the determination of the power required for operating the train. A specific instance will show this most clearly. Take the case of a double truck car weighing 30 tons loaded. It has eight 33-in. wheels weighing about 700 lbs. each, and two motors rated, we will say, at 150 h. p. each. The radius of gyration of the wheels is about 77 per cent. of the radius of the wheel. The center gyration of the wheel, therefore, moves at a rate 77 per cent. of that of the train. The flywheel effect of each wheel then is equal to a weight of $\frac{700}{.77^2} = 415$ lbs. when reduced to the speed of the car. Eight wheels will therefore add 3,320 lbs. to the inertia weight of the car.

The armatures have each a flywheel effect of 1,400 lbs. at a radius of 6 inches. With a gear ratio of 18 : 53 the center of gyration of the armature will move a distance of 9.25 feet for every revolution of the axle, or for a corresponding movement of the car of 8.6 feet. Its relative speed is, therefore, $\frac{9.25}{8.6} = 1.08$ times the car speed and the flywheel effect is therefore $1{,}400 \times 1.08^2 = 1{,}640$ lbs. reduced to the car speed. The two armatures thus add an equivalent weight of 3,280 to the inertia weight of the car.

The wheels and armatures together add an equivalent of 3,320 + 3,280 = 6,600 lbs., or about 11 per cent. to the inertia weight of the car.

The following paragraph will show the effect of a change in the gear ratios :

With a gear ratio of 20 : 51 the flywheel effect of the armatures would be equivalent to the addition of 2,400 lbs. to the inertia weight of the car. This together with that of the wheels adds a total of 5,720 lbs. or about 9.5 per cent. to the inertia weight of the car.

From a considerable number of instances that have been taken the flywheel effect of the rotating part of an electric car is found to average about 10 per cent. of the inertia of the entire weight of the train. This means that 10 per cent. more energy is stored in every train than is accounted for by the dead weight; 10 per cent. more power is required for accelerating, 10 per cent. more energy is lost in braking, and the train resistance measured by the retardation in coasting is 10 per cent. below the true resistance.

The actual increase in energy supplied to a train on account of the flywheel effect of the rotating parts is the energy in these parts which is lost in braking. The relation this bears to the total power developed by the motors is dependent on the number of stops, the speed at the time the brakes are applied and on the energy absorbed by the train resistance. Where the stops are frequent, the energy lost in brakes may be from 50 per cent. to 75 per cent. of the entire power developed by the motors, in which case the energy required by the rotating parts will be from 5 to 7½ per cent. of the total.

There are two simple methods for including this item in the calculations. The first is by basing the calculations on a weight of car 10 per cent. heavier than the actual in determining the acceleration and drifting. The second is by assuming that the force required to produce a certain rate of acceleration is 10 per cent. higher than would be necessary if it were simply a dead weight. This is probably the simpler method, as it gives round numbers for calculations, as 10 per cent. added to 91.3 gives practically 100 lbs per ton as the force required for accelerating at the rate of one mile per hour per second. All that is necessary then in correcting calculations for accelerations is to use this figure of 100 lbs. per ton instead of 91.3. It will give a good average correction, although if great accuracy is desired it will be preferable to calculate the flywheel effect of each of the rotating parts of the train separately. It will usually be found that for slower speed service, where the gear reduction is considerable, that more than 10 per cent. will be required, while for high speed interurban work where it would really amount to very little anyway on account of the small number of accelerations, the amount to be added for the correction will be less than 10 per cent.

As will be readily recognized this factor will also enter into the determination of the train resistance from the coasting line. Just how much of a correction will have to be made on this account depends somewhat on what is considered to be the train resistance. If this includes the friction in the motor then the train resistance obtained from the coasting line will be 10 per cent. lower than the actual train resistance. On this assumption if the retardation in coasting is .2 miles per hour per second, the train resistance will be 20 lbs. per ton instead of .182 as calculated by the ordinary method. If, however, the train resistance

does not include the friction of the motor the correction necessary to be made for inertia of rotating parts will be small, because it will be nearly balanced by the motor friction. When the train is coasting, the inertia of the rotating part is added to the inertia of the dead weight of the train in tending to keep up the speed, while the friction of the motor is tending to reduce the speed. When the train is accelerating, or moving with power on the motors, the motor friction is taken into account in the efficiency curve of the motor so that the train resistance to be used in calculating the acceleration should properly not include the motor friction. It will thus be seen that the error due to motor friction and the inertia of the rotating parts will tend to counterbalance each other in the determination of the train resistance from the coasting line. For accurate determinations, however, the train resistance should include a consideration of both the inertia of the rotating parts and the motor friction. The most accurate way to obtain this is to plot the friction curve of the motor and to obtain the inertia of the armature and wheels either from tests or from calculations based on the drawings.

It may be considered that this is an undue refinement, but if the matter is carefully investigated it will be found that the motor friction is quite a considerable portion of the total train resistance, in the same way as the inertia of the rotating parts is a considerable portion of the inertia of the train.

It is understood that general solutions for the railway problem may be offered which will give fair approximations of the motor capacity and the amount of power required. It is understood that any good engineer with a fair amount of experience can give a pretty good estimate of the power required for a given service even when considering the capacity of the motor according to the old horse power rating. But where accuracy is required, every known element should be considered at its proper value and there will still be at best enough variable quantities in the railway problem. The most reliable estimate for train resistance should be used; the inertia of the rotating parts should be obtained and considered; the weight of the entire train should be known; the acceleration and power curves should be carefully plotted and the average heating effect in the motors should be accurately determined before any large equipment is finally decided upon.

In discussion of Dr. Hutchinson's Paper, January 24th, 1902.

THE SELECTION OF ELECTRIC MOTORS FOR RAILWAY SERVICE.

BY W. B. POTTER.

The excellent paper by Dr. Cary T. Hutchinson, on the subject of energy and motor capacity for electric trains, shows a careful study of the problem, considered from an average basis. In discussing that paper I shall endeavor to point out some variations from the average met with in the practical consideration of motor selection.

It may be stated as a general proposition that for a given schedule the lowest power consumption will be secured with the lowest speed gearing that will ensure with reasonable margin the performance of the desired schedule. The torque per ampere is proportionately greater with the low speed gearing, and the maximum speed is also less; the first with respect to current fluctuations, due to improper handling of the controller, and the latter with respect to energy consumption, as influenced by the percentage of coasting, being less affected in their lower values by unskilled motormen.

Motormen will frequently keep the power on until they apply the brakes, regardless of the requirements of the schedule, and if gaining on the schedule will apply the brakes to slow down midway between stops instead of allowing the car to coast, as they should do before reaching maximum speed. It is a well known and proven fact that different motormen, by reason of different percentages of coasting, may make a difference of at least 20% in the energy required for the performance of a given schedule. The less frequent the number of stops, the less will be the difference between different men. The difference may be taken ap-

proximately as 2% for each stop per mile, and the more nearly the calculated requirements approach the theoretical or best possible economy, the greater the allowance that should be made, as it is a fair assumption that none of the cars will be handled in a manner to secure the highest possible efficiency.

When a close calculation of the energy is desired, it is of advantage to use the actual characteristic of the motor under consideration rather than an average characteristic, not so much on account of the difference in power, due to the characteristic alone as on account of the shape of the speed curve, which may differ widely at free running speed with different types of motors.

The free running speed assumed by Dr. Hutchinson as 130% of the speed at which the external resistance is cut out, would imply that power was cut off before the car had attained full acceleration, and it would depend entirely upon the service requirements whether such would be the case. Even 160% is not sufficient for some motors. The free running speed will vary from 150% to 200% the speed at which the resistance is cut out, that is, with motors of low saturation the car may double its speed on the motor curve.

The determination of motor capacity, that is, the motor heating in a given service, cannot with desired accuracy be obtained from the average of a number of motors. Dr. Hutchinson has assumed an average copper loss of 8.6% of a rated input. This will vary from 5% in the larger motors to 12% in smaller motors, and different motors of the same commercial horse power rating may vary 30% to 40%.

The average core loss has been assumed as 3.4% of the rated input, and this percentage between different motors will be found to vary between 2% and 5%, and with motors having the same commercial horse power rating, the core loss of some will be found to be double that of others. The following table will indicate some of the differences that actually exist between the motors of several different manufacturers.

PERCENTAGE LOSSES OF INPUT AT RATED CAPACITY.

Commercial Rating.	Field.	Armature.			Motor Total.
	I^2R	I^2R	Core.	Total.	
38 H. P.	4.70	4.00	2.37	6.37	11.07
38 "	4.60	3.80	4.92	8.72	13.32
50 "	4.20	2.10	3.45	5.55	9.75
50 "	4.33	3.36	4.17	7.53	11.86
50 "	3.25	2.80	4.80	7.60	10.82
75 "	3.20	2.50	2.93	5.43	8.63
125 "	2.48	2.40	2.12	4.52	7.00

The average service losses in the motor as a whole have been assumed as an average of 9.4% of the rated input, based on a maximum speed of 130%. This average, both with respect to percentage and maximum speed, will vary materially with the character of the service; on frequent stops it may be a total of 9.6% with 3.2% in the field, and 6.4% in the armature. With the same motor and infrequent stops the loss would be more nearly 3.4% with .6% in the field, and 2.8% in the armature. As between different motors of the same rating for the same schedule the total loss may be found to vary 30% or more.

As between speed at rated capacity and free running speed the core loss of some motors is constant within 5%, while in other motors it may vary 100%, being less at full speed.

The assumption that a motor will run one-quarter of the time at its commercially rated input for its rated temperature rise is a fair average, but actually the time will vary from 20% to 50% of the total time and depends on whether the motor is running closed or open, on the stand, or under service conditions. In consideration of the variations that exist between different motors, it would seem a reasonable assumption that calculated temperatures based on the average of a number of motors would not give the actual loss in the fields within approximately 20%, or the loss in the armature within approximately 40%, and combining these variations with the difference in radiating capacity of different motors, the result based on averages might not give the actual temperature rise within 100%.

The assumption that a motor as a whole will radiate continuously 3% of its rated input is also a fair average, the variation being approximately 2.5% to 3.5%. This, however, does not give the distribution of losses in the field and armature, which is important as affecting temperature rise, and from the above table it will be noted that the distribution differs widely with different motors.

The commercial basis of rating a railway motor, *i. e.* "The horse power output giving 75 degrees C. rise for one hour's continuous run at 500 volts on a stand with covers off," was selected several years ago by the writer after a careful consideration of what the commercial rating of a railway motor could reasonably be expected to cover.

This method serves the commercial necessity of a relative measure of capacity between different motors, and the current re-

quired to give a temperature rise of 75 degrees C. in one hour insures a satisfactory test of commutation. It is further a desirably severe test of the mechanical and electrical design of the motor.

A criticism of this method of rating might be that the voltage should preferably be 550 instead of 500 volts, as 550 volts is now the more common voltage.

Seventy-five degrees C. rise was purposely taken as a higher temperature than railway motors should be operated in service, sixty-five degrees C. rise as a maximum and 55 degrees C. as an average being as high a temperature rise as is compatible with reasonable maintenance.

Before selecting this method the subject was carefully studied with reference to giving a significant rating applicable to service conditions, but the difficulty of doing this will be appreciated when it is understood that a tabulated statement of any one motor with different gear ratios, weight of cars and stops per mile includes one hundred or more different schedules.

The use of motors too small for the service, with resultant burning out from overload, is inexcusable, for not only are they expensive to rewind, but the delays and annoyance to passengers incident to break down is above all things to to be avoided. Reliability of service should be the primary consideration in every case.

The equivalent heating or reproduction of a given service by running the motor at the current giving an equivalent I^2R loss, and at the voltage giving the average core loss would be convenient and correct, providing the effective amperes of the given service are determined and applied at the proper voltage. The voltage at which such test should be made representing in effect the particular core loss for a given service will vary widely with motors of different capacities and even between motors of the same capacity having different core losses.

Such a method assumes that for a common service there is a certain current and voltage common to all motors of the same capacity, which, if run on a stand with this current and voltage, will produce in the motors equivalent losses and in their proper distribution, and that the temperature rise of the armature and field so obtained will closely approximate the temperature rise in actual service.

The speed curves of railway motors materially differ; that is, they have different saturation curves, and, as before mentioned, the maximum free running speed in motors of the same rating may differ 50% on the motor curve. Even assuming that the energy input in two cases is the same, the rate of input will not be the same, due to variations in the saturation curve; that is, the average current will be the same, but the current as effecting the heating, which is the square root of the mean square, will not be the same, unless motors have the same saturation curve. These variations lead to the conclusion that a common current cannot be taken for various motors for a given service, as, regardless of the gear ratio, the speed, torque and ampere constants of each motor will not coincide, unless they have a speed curve of the same shape, which is seldom the case.

As before mentioned, the core losses of motors having the same commercial rating may vary as much as 100%, some core loss curves being nearly constant in watt loss throughout the working range of the motor, while others at rated load may be double the watt loss at full speed.

The core losses are composed partly of eddy currents varying as a square of the speed or voltage, and partly of hysterisis, varying directly as the speed or voltage, and the relative proportion of these two losses will vary greatly in different motors. It is not likely between different motors that the core loss of each will be composed of eddy currents and hysterisis in the same ratio; thus the speed or voltage that would give the service core loss would be quite different with different motors.

In order that the same current and voltage shall apply to several motors of different design or manufacture for a given service, it is necessary that the motors be geared to exactly the same speed, that the motors shall have identically the same efficiency throughout, that the shape of the speed curve or saturation be identical, that the core loss curve be of the same shape, and be composed of eddy and hysterisis losses in the same proportion.

It is needless to add that such uniformity does not exist, and it only remains to determine how far the difference between similar types of motors of different manufacturers differ from each other. This is shown by the following table of motors of different manufacture, with the current and voltage to give the losses and their proper distribution, the motors being geared

to make the same schedule. The acceleration is taken at 1.5 m. p. h. per second in every case.

Distance, 750 feet.	Time, 54.4 seconds.	
Commercial rating.	Amperes.	Volts.
35 h. p.	35.9	310
35 h. p	33.6	344
38 h. p.	32.4	373
38 h. p	33.4	351
Distance, 1 mile.	Time, 227 seconds.	
35 h. p	24.6	478
35 h. p	23.4	452
38 h. p.	22.4	458
38 h. p.	23.4	377

The above values for current and voltage would vary considerably with different percentages of coasting, and different schedules.

Owing to the complex character of the core losses and speed curve a large number of calculations, of which the above are representative, indicate the futility of comparing or determining motors by assuming an arbitrary voltage. The current and voltage must be predetermined for each particular motor and service.

This method of rating further assumes that the temperature rise and the distribution of temperatures in a motor run on the stand will hold good under service conditions. Experience does not indicate this to be the case. A motor with a ventilated armature will give more nearly the service temperature rise on a stand than a motor with poorly ventilated armature. Furthermore, with a large number of starts per mile, the motor is run either at relatively slow speed or not running at all for a considerable portion of the time, while with few stops on high speed suburban service the motor is relatively cooled by the long continuous run, with the result that in the first instance the stand tests may give lower, and in the latter higher, temperatures, due to the relative difference in radiation of the motor frame.

For such determination of the motor capacity as is essential to the recommendation of motors suitable for a given service, experience has shown the desirability of determining the losses and consequent heating of the field and armature independently for each particular case.

From a speed curve based on the acceleration and maximum speed required for a given schedule the proper gearing, winding

and commutation of a motor suitable for the service can be obtained. Owing to different lengths of runs, slow-downs at curves, and the effect of grades, the speed curves of different runs over the line may, and usually do, differ materially. Having established the speed curves for a given service, the field and armature losses may be integrated by reference to the resistance and core loss curves of the motor under the varying conditions of control and running on the motor curve.

Having determined the average loss in the field and armature for a particular service, the next step is to determine the temperature rise of the field and armature under these conditions.

It is obvious that the most accurate means for determining the heating effect for different losses is to test the motors under service conditions at a known loss in the field and armature, and to measure the actual resultant heating.

To this end the motors should be mounted on a car and operated at various schedules and distances on a track, careful records being taken with recording instruments of the speed-time curves, amperes and voltage, the tests being continued until the motor temperature is constant. The temperature should be taken both by thermometer and by resistance, both with the motors closed and with the covers removed. From the known losses in the motor during these tests there may be determined the degrees rise per watt loss, both for the field and armature; the distribution of the losses in the two elements being represented by the watt loss in the armature divided by the watt loss in the field.

Having determined the ratio of losses for the particular service a reference to the temperature tests for the particular motor and ratio of losses enables one to determine the degrees rise per watt loss for the armature and field. It is then a simple matter of multiplication to determine closely the temperature rise of the armature and field for the proposed service.

Several years' experience with different methods of determining motor capacity have shown this to be the only rational and reasonably accurate method, as it takes into account all the variable factors.

The following table has been calculated directly from results determined under service conditions as described above, and shows in tabulated form the capacity for a safe temperature of a 38 h. p. motor with different stops per mile, gear ratios and tons per motor.

LIST OF SCHEDULE SPEEDS FOR 88 H. P. MOTOR.

500 Volts. 33 in. Wheels. Straight and Level Track. 10% Margin Allowed in Schedule.

Stops per Mile.	Gear Ratio.	TONS PER MOTOR.									
		4	4.5	5	5.5	6	6.5	7	8	9	10
1/4	4.60	22.3	21.7	21.2	20.9	20.5	20.2	19.8	19.3	18.8	18.5
1/4	3.94	23.3	22.9	22.4	22.0	21.7	21.3	21.0	20.4	19 9	.....
1/4	3.42	25.0	24.6	24.2	23.7	23.2	22.8	22.4	.....	.....	.....
1/4	2.82	27.6	27.0	26.5	26.0	.....	.....	.....	.....	.....	.....
1/2	4.60	20.9	20.5	20.2	19.8	19.5	19.3	18.7	18.2	17.6	17.3
1/2	3.94	22.0	21.6	21.2	20.9	20.5	20.2	19.8	19.2	18.5	.....
1/2	3.42	23.5	23.0	22.6	22.2	21.8	21.3	21.0	.....	.....	.....
1/2	2 82	25.6	25.0	24.6	24.0	.....	.....	.....	.....	.....	.....
1	4.60	19.4	19.0	18.6	18.3	18.0	17.7	17.5	16.8	16.4	15.7
1	3.94	20.3	19.9	19.6	19.3	18.9	18.5	18.2	17.5	16.8	.....
1	3.42	21.6	21 2	20.7	20.3	20.0	19.6	19.3	.....	.....	.....
1	2.82	23.0	22.6	22.1	21.6	.....	.....	...	.....	.....	.....
2	4.60	15.8	15.5	15.3	15.1	14.9	14.7	14.6	14.2	13.9	13.5
2	3.94	16.6	16.4	16.1	14.9	15.7	15.5	15.2	14.8	... ..	.....
2	3.42	17.6	17.2	16.9	16.7	16.3	16.0	.....	.....	.....	.....
2	2.82	18.1	17 8	17.6	... ..	.....	.....	.....	.....	.....	.....
3	4.60	13.7	13.5	13.3	13.1	13.0	12.9	12.8	12.5	12.2	12.0
3	3.94	14.3	14.1	13.9	13.8	13.7	13.5	13.3	12.9	.....	.....
3	3.42	14.9	14.8	14.6	14.3	14.0	.....	.....	.....	.....	.....
3	2.82	15.4	15.2	.....	.....	.....	.....	.....	.....	.....	.....
4	4.60	12.2	12.1	12.0	11.8	11.6	11.5	11.3	11.2	11.0	10.8
4	3.94	12.6	12.4	12.3	12.2	12.1	12.0	11 8	11.4	.....	.....
4	3.42	13.0	12.8	12.7	12.5	12.3	.....	.....	.....	.....	.....
4	2.82	13.3	13.1	.....	.....	.....	.....	.....	.....	.....	.....
5	4.60	11.1	11.0	10.9	10.8	10.7	10.6	10.4	10.3	10.1	.....
5	3 94	11.4	11.3	11.2	11.1	11.0	10.9	10.7	10.4	.....	.....
5	3.42	11.8	11.6	11.5	11.3	.....	.....	.....	.....	.....	.....
5	2.82	11.9	.....	.....	.....	.....	.....	.....	.....	.....	.....
6	4.60	10.1	10.0	9.9	9.8	9.8	9.7	9 6	9.5	9.2	.....
6	3.94	10.4	10.3	10.2	10.1	10.0	9.8	9.7	.....	.....	.....
6	3.42	10.6	10.5	10.4	10.3	.....	.....	.....	.....	.....	.....
6	2.82	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
7	4.60	9.4	9.3	9.2	9.1	9.1	9.0	8.9	8.8	8.5	.....
7	3.94	9.6	9.5	9 5	9.4	9.4	9.2	9.1	.....	.....	.....
7	3.42	9.7	9.6	9.5	.....	.....	.....	.....	.....	.....	.....
7	2.82	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
8	4.60	8.8	8.7	8.6	8.5	8.5	8.4	8.4	8.3	8.2	.....
8	3.94	9.0	8.9	8.8	8.8	8.7	8.6	8.5	.....	.....	.....
8	3.42	9.0	8 9	8.9	.....	.....	.....	.....	.....	.....	.....
8	2.82	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
Max. Speed	4.60	27.3		26.0		25.0			23.5		22.7
	3.94	29.0		27.5		26.5			25.0		24.0
*Double Equip.	3.42	31.0		29.6		28.5			26.9		25.9
	2.82	35.0		33.5		32.3			30.6		29.5

*Speed of four motor equipment 5 to 10 per cent. faster for same tons per motor.

Such a method serves as a rapid, convenient and reasonably accurate method of selecting a motor within the variations covered.

This practical determination of the heating factors has led to marked improvements in the electrical design of motors, and the

value and utility of these tests under service conditions can hardly be over-estimated in their application to practical conditions.

In closing I wish to express my thanks to Messrs. E. D. Priest and E. H. Anderson for their assistance in the calculations incident to the preparation of this paper.

Schenectady, N. Y., Jan. 23, 1902.

Discussion.

Papers by Wm. S. Aldrich, F. A. C. Perrine, Charles F. Scott, Charles P. Steinmetz and E. W. Rice, Jr., Buffalo, August 22d, 1901.

Vice-President Dunn [*Chairman*]:—As was our practice yesterday, all of the papers will be thrown open for discussion simultaneously. Mr. Steinmetz, on account of presenting a paper himself and having read Mr. Rice's paper, has asked me to remain in the chair during the discussion. The papers are now thrown open for discussion, which it is hoped will be full and profitable.

Prof. Geo. D. Shepherdson: The point brought out by Dr. Perrine, that the loss by the brush discharge depends upon the radius of the curve of the conductor, indicates two or three interesting conclusions. One is that a solid conductor is preferable to a stranded conductor; at least, after being once erected. Of course, it may involve difficulties in erection when we get into large sizes. Another conclusion is that after a certain point is reached aluminium will be much preferable to copper, on account of its larger size. That may be of importance in two ways; one is that higher voltages may be practicable with aluminium conductors than with copper; the other point is that it may be practicable to transmit small powers over a greater distance with aluminium than with copper. Of course, that is modified by the relative costs of copper and aluminium, but a case might very easily arise where aluminium would cost more, relatively to copper than it ordinarily does, and yet it might be preferable on account of the possibility of carrying power farther at a higher voltage, or of carrying a smaller power over a great distance.

Mr. Henry W. Fisher:—I have been very much interested in Dr. Perrine's paper, especially with reference to the flattening of the e. m. f. curve due to capacity, particularly so as I made some tests a number of years ago which seemed to show the other effect, and I thought it might be interesting to tell how these tests were made. A 2500 k. w. generator was used, operated by the Allegheny County Light Company of Pittsburg. From this generator a 2000-volt circuit led to an auto converter and was taken from the auto converter to a high-voltage converter, and from the secondary of said converter to some lead-covered cables, one side being connected to the conductors and the other to the lead sheath. The instruments used for getting a comparison between the average and maximum voltages were a Weston voltmeter and a spark-distance measuring apparatus. The spark-distance apparatus was placed across the high voltage end of the converter and the curve for spark distances found up to 10,000 or 12,000 volts. Having determined the above on the same evening, with the same generator, all the conditions being

as nearly alike as possible, connection was made to about 3500 feet of cable having a capacity of, say, .2 or .3 microfarad, and it was then observed that the spark distances were greater for given voltages. In order to make sure that there was not some change in the outside circuit the cable was cut off and the same tests made without any capacity. The spark distances were the same as at first. This leads to a very important fact. If capacity on the line does flatten the e. m. f. curve, the spark distance must be affected by this capacity to the extent of increased spark length for decreased maximum voltage. Or else it may be that the sharpening or flattening of the e. m. f. curve varies with capacity, self-induction, etc., and with a small capacity it might have one effect, with a larger capacity the other.

The experiments were conducted so carefully that I am certain under the existing conditions the e. m. f. curve was made sharp. Experiments were then conducted with the same generator at different hours of the evening, and it was found that when the generator was heavily loaded in the early part of the evening the spark distances were smaller; and later in the evening, when the generator was not so heavily loaded, the spark distances were larger. With the capacity in multiple the spark distance was increased about 10 per cent. I wished to bring up these points because I thought some one might be able to explain this apparent difference between Prof. Perrine's results and those obtained as shown.

MR. W. L. R. EMMET:—There are two statements that have attracted my attention in these papers which seem to imply a certain limit of the usefulness of electrical apparatus through possibilities of trouble which may occur in its use. I call attention to them in the general interest of electrical engineering, One of them was in Mr. Scott's paper. He rather implied that it was necessary to design dynamos, motors and rotary converters to suit one another; that the synchronous motor must be adapted to the condition of its operation, and that if the voltage was not exactly adjusted it gave rise to troublesome, expensive or destructive fluctuations of current or voltage in the system. I do not think that in practice those troubles have ever existed. Neither does it appear necessary for any skilled manufacturer of such apparatus to say to a producer, "This is only adapted to use with motors of a particular design, and with certain kinds of rotary converters, and if your conditions are altered and you have to put something else on your system, troubles will arise." Take, for example, the action of the synchronous motor with variations of the e. m. f. wave in a generating plant, or variations of speed in a system, or variations of voltage in a system. In the case of variation of the e. m. f. wave the differences between the inherent wave of the synchronous motor and of the generator, are ordinarily differences of considerably higher frequency than the main wave of the system. They are simply overtones which are

largely obliterated by the effects of self-induction, and the worst that can result are certain cross currents in the period of those overtones. These are so small relatively to the main oscillations of the system, that they are inappreciable; and in practice rotary converters and synchronous motors operate all over the world, on all sorts of e. m. f. waves, and under all sorts of variations and conditions, and the troubles are apparently as infrequent where there is no conformity of wave as where there is perfect conformity. Practical experience teaches us that synchronous apparatus, operating in parallel is, in spite of these theoretical difficulties, quite rugged and serviceable where the conditions of design are proper. Another matter which has been unduly dwelt upon by Mr. Scott is the importance of adjusting synchronous motor fields to conform to change of voltage on the line. The synchronous motor with fixed excitation tends to act as a steadier of the system, its variation of power factor with change of load being very small when the motor is ordinarily proportioned. While everything that Mr. Scott says about the usefulness of the induction motor is true, there are, as he also admits, wide fields for the use of the synchronous motor. In practical service the troubles which he has mentioned are not often sources of interruption.

The other point referred to was in Mr. Steinmetz's paper, where he mentions as incident to the use of certain circuit opening devices variations of voltage which are prohibitive and which cannot be provided for by insulation. There is a fact in connection with this matter that is of great practical importance; this is, the power of resisting such excessive strains which is possessed by certain types of insulation. Air in one sense, the simplest sense, may be considered the most perfect insulator. Dry air is a very perfect insulator. It requires very high voltage to be punctured. The puncture of air, however, is something which involves no expenditure of energy. It is instantaneous and can occur simultaneously with the existence of the voltage capable of jumping that distance; whereas solid insulators, which are continuous and free from porosity and from openings, or from air passages, require considerable expenditure of energy to puncture. The excessive voltage impulses to which Mr. Steinmetz alludes are of very high frequency and do not continue long enough to effect the disintegration of the insulation, and as a consequence we see transformers short circuited through air across their terminals several feet apart, where the insulation is entirely unaffected. In point of fact, in some of the systems where the most trouble has occurred with rupturing of air insulation, arcing across the backs of switchboards and to adjoining pipes and structural work, there has been no trouble whatever with the insulation of the system. On this Niagara Falls system, there have been at times tremendous manifestations of e. m. f. about the connections at the terminal houses, indicated by the

jumping of wide air-gaps, yet there has never been at such times any case of trouble with the insulation of the transformers, of any make. Furthermore, we see in lightning storms tremendous potentials applied to the lines, and yet no trouble with the insulation if it be of a certain character. If the insulation is such that air is depended upon, no matter how great the distance over the terminals, trouble is liable to occur under certain circuit conditions, for example, where a transformer has two exposed terminals three or four feet apart, it will short-circuit across those terminals through the air, but if it is properly constructed and suitable methods of insulation used, it will not be injured. A great deal can be accomplished in a practical way by observing those conditions and building apparatus to meet them.

DR. A. E. KENNELLY:—With regard to Mr. Scott's paper, which gives a distinct and useful differentiation between synchronous motors and induction motors, I think the comparison is somewhat severe to the synchronous motor. Only a few years ago we were very glad to have the synchronous motor, before the induction motor made its appearance, and now that the synchronous motor has its duties largely replaced by the induction motor, I think it is a little unfair to an old friend to underrate it. Moreover, in the mixed systems of long distance distribution I think it is desirable to employ both synchronous and induction motors, each in their proper spheres of action. Without synchronous motors the power-factor of a system may be unduly low.

I would also like to put in a plea for dropping that unfortunate word "rotary," in connection with converters. It seems to have become fixed in our minds that a converter must be a rotary converter. Of course, a converter, as we understand it, must go round; but would it not be possible on an occasion like this to take some steps for dropping an unnecessary word which plays no useful part?

As regards Mr. Steinmetz's very interesting paper, I think that it shows, from a theoretical standpoint, the importance which may attach to an evanescent constant in an algebraic equation. The evanescent constant which occurs in every alternating circuit equation, which we usually ignore and regard as vanishing after a few cycles, may, as he points out, rise to a prominence 100 times greater than the fundamental, and in such a case one cannot afford to ignore the evanescent constant. Its practical importance, of course, attaches to the great danger which may result from the short circuit of alternating-current mains under particular conditions. We believed that we had enough trouble with short circuits on direct-current generators, and when we had alternators which would refuse to give excessive currents on short circuit, we felt that we were comparatively free from dangers from short circuits on alternating-current systems, but owing to the surges which may occur in such

systems we have a new danger, very formidable, under particular conditions. The difference between the surges which occur in the case of such a short circuit and the resonance which ordinarily may occur in the normal operation of a line is very interesting. In the one case the resonance in the normal operation of a line is dependent upon the harmonics in the e. m. f. wave, and if all harmonics were removed from the e. m. f. there would be no resonance. The case is one of forced oscillations. Certain harmonics are present in the applied e. m. f. which will force certain effects upon the transmission system under particular conditions. In the other case, of which Mr. Steinmetz has now made an analysis, and which he brings to our attention so lucidly, it is not a question of harmonics, but simply a question of the oscillations of a large system of conductors containing inductance and capacity; and no matter how these conductors may be charged, or the kind of wave employed, or the source of e. m. f., these dangerous oscillations may be developed under certain conditions attending sudden changes of current or pressure. A very curious instance is brought to our attention by Mr. Steinmetz himself in pointing out that the Wehnelt interruptions offer an analogy to these oscillations.

PRESIDENT STEINMETZ:—Mr. Chairman, Members of the Institute and Guests—I desire to say a few words regarding Mr. Scott's paper. While I can fairly agree with the second part of it I am sorry to be obliged to disagree from a considerable part of the first portion, giving a comparison between synchronous and induction motors. Whatever I shall say, and with all deference to Mr. Scott, refers to motors of larger sizes, since we all agree that in small, and very small sizes, the induction motor is preferable by reason of its simplicity, due to the absence of the exciter, and since in small motors the objectionable features of the induction type are not so serious and can be eliminated to a large extent by the judicious use of some large synchronous motors in the same system. I have controlled the design of some hundred thousand k. w. of induction motors and some hundred thousand k. w. of synchronous motors. I believe I have been fairly successful with either type, and so have no preference for one or the other design; but some years ago, when a discussion came up on this topic before the INSTITUTE, I made the statement that "I see no excuse ever to use an induction motor where a synchronous motor can be used," and my experience has not yet shown me any reason to change that opinion.

Coming now to the parallel columns, I must not only take exception to some things stated, but also add that a number of points have been omitted which appear to me essential for a fair comparison, as for instance, that there are two essentially different forms of induction motors, the "squirrel cage" motor which is ideal in its simplicity, but requires the same excessive starting current as the synchronous motor, and the rheostatic motor which

consumes in starting the same current as when running with the same load, but requires rheostat and collector rings and brushes or switches. Obviously only a few of the statements can be discussed.

Regarding auxiliary apparatus, the "squirrel cage" induction motor, if required to start at moderate current, needs a starting motor just as well as the synchronous motor. The difference between the "some form of transformer for reducing the voltage" in the synchronous motor, and the "auto-transformer for reducing the e. m. f. for starting" in the induction motor, I fail to see.

Exciter and field rheostat are indeed not required for the induction motor, but armature rheostats with switches or collector rings are required if the induction motor shall give a good starting torque with moderate starting current.

All the statements regarding adjusting the field currents, which are repeated several times, are greatly exaggerated. With any reasonably fair synchronous motor, without adjustment of the field excitation but at constant excitation at all loads, the power factor will never fall as low as it is with the induction motor at the same load.

No "squirrel cage" induction motor with reasonable efficiency starts at full load excepting with currents many times larger than full load current, so that the "squirrel cage" induction motor should be started light, the same as the synchronous motor.

In the construction, the "field winding of many turns liable to accident" is in the synchronous motor not different from the field winding of a low voltage, continuous current machine, and the latter is considered nearly as safe as any piece of electrical machinery.

Collector rings and brushes or switches, and in addition a rheostat, are required in the induction motor if starting torque with moderate starting current is desired.

Here it should be added that the synchronous motor has the advantage of a good mechanical construction with a generous air-gap between stationary and revolving parts, while the induction motor to have a reasonable power factor must run with microscopic clearance between stator and rotor.

The synchronous motor is started by throwing a switch in starting, and then in running position, like the induction motor, and then gives the same small torque with large current. The only additional feature is that after the start the field switch of the synchronous motor is closed. The synchronous motor can, however, also be started with small current by the use of a starting motor which brings it to synchronism.

Neither the induction motor nor the synchronous motor when thrown out of step by overload will recover, excepting when the load is thrown off.

The starting of the synchronous motor is no more inefficient than that of the "squirrel cage" induction motor, and the maxi-

mum torque of the synchronous motor is frequently greater than that of the induction motor.

No alternating current motor can operate efficiently at fractional speeds except by changing the frequency or the number of poles, etc., and the synchronous motor, as well as the induction motor, is essentially synchronous regarding speed.

Regarding current consumed in starting, I have never seen or heard of a large and efficient "squirrel cage" induction motor giving full load starting torque with $1\frac{1}{2}$ to $2\frac{1}{2}$ times full load current. Four to six times is as a rule nearer the facts.

The wave-form as a factor in explaining misbehavior of alternating current apparatus is rather discredited nowadays. While the influence of the wave-forms on the current can be observed in the induction motor, even more than in the synchronous motor, especially when running light, this feature becomes negligible at load.

The next paragraph of Mr. Scott's paper is the only consideration regarding the decision between synchronous and induction motor which I consider justified. It means that the use of the synchronous motor is restricted by certain limitations regarding steadiness of frequency, resistance of supply lines, etc. Where these limiting conditions are fulfilled, the synchronous motor does not hunt and is preferable to the "squirrel cage" induction motor. Outside of the limits of operativeness of the synchronous motor the induction motor must be used. Obviously where excessive starting torque and small starting current is needed the induction motor with variable armature resistance is appropriate.

As stated before, without adjustment of the field current, synchronous motors give power factors higher than induction motors, but by varying the field current the synchronous motor can be made to compensate for objectionable lagging currents produced by other parts of the system, as by induction motors. It is true indeed that the synchronous motor represents an active or live e. m. f. This frequently is one of its most valuable features, that, with the supply of electric power failing, the synchronous motor, and also to a lesser extent the converter, driven by engine power, is ready to turn generator, and so becomes the generator reserve of the sub-station, supplying power to lights, induction motors and other dead loads on the system. This feature is utilized sometimes to such an extent that during the daily changes of load the synchronous motor regularly changes from motor during daytime to generator during the peak of the lighting load.

Regarding the summary, most of the statements have already been discussed. It should be, however, added here that the synchronous motor is smaller than the induction motor, more reliable in its operation, and is of a higher efficiency. It can be wound directly for voltages of 10,000 and more, which the

induction motor cannot, and step-down transformers and the complication and loss of power inherent thereto can be saved, so that considering that the synchronous motor alone already exceeds in efficiency the induction motor, when compared with induction motor and transformer, the high voltage synchronous motor is vastly superior in efficiency. In operation and adjustment it requires no more but rather less skill than the rotary converter recommended in the last part of the paper under discussion. It appears, therefore, that the synchronous motor and not the induction motor is the type especially suited for transmission and distribution of power in large units, and the induction motor finds its proper field in power distribution in small and moderate sizes and for special applications, such as frequent starting under heavy torque, and then as a special motor.

Now, from all this, the conclusion I derive is that the synchronous motor is by no means the decadent apparatus, that Mr. Scott apparently states, but is not only alive, but is preferable as a type of machine; that it is the machine which should be used wherever possible; but that there are certain limitations outside of which the induction motor must be used because there is nothing better.

As to this very high power factor of the induction motor, we must consider that even if the power factor in special cases, with a special low frequency motor, exceeds 95 per cent., the average running power factor probably does not exceed 90 per cent., but if anything is lower. A 90 per cent. factor means 44 per cent. inductance factor or 44 per cent. of lagging alternating current and a corresponding reaction on the system and on the regulation of the generator.

DR. F. A. C. PERRINE:—I wish to add something to President Steinmetz's remarks, from my own experience as an operator of induction motors. From that experience I would say that the advantages set forth in the paper are largely possible advantages, and while any one of them may be found in a particular case, the conclusions drawn do not generally apply together. As regards the point made by the President concerning starting torque with $1\frac{1}{2}$ times full load current, I know of one case where a 400-h. p. induction motor was built and guaranteed to start with $1\frac{1}{2}$ times full load current, the contract between the user of the current and the transmission company having been made to require it. In that case it was necessary to insert four friction clutches between the motor and the flour mill operated by it. The first friction clutch was between the line shafting and the rolls, and so on; all of which was necessary in order to start up that induction motor and keep the starting current down to $1\frac{1}{2}$ times full load current. In another case, also in a flour mill, where a high efficiency induction motor was demanded, the starting current was so great that a 450 k. w. generator would not start a 300 h. p. induction motor. The water-wheel attached to the 450

k. w. generator was slowed down by the power demanded to start up the motor, and in addition it was found necessary to use a number of men pulling on the belt. At first the superintendent said it was a good thing, because he never had had his men on time before. With the motor in service they had to come early in order to pull on the belt and start up the mill. This was finally found to be a burden, and the owner demanded that the induction motor be re-designed so that it could start up alone. The manufacturer changed the end rings on the "squirrel-cage" armature, and the motor would then start the mill without any difficulty. But the mill was paying for its power by wattmeter, and the cost of the power was increased by one-half cent a barrel of flour on account of the decreased efficiency of the motor.

And now, as regards the impossibility of running transmission plants with synchronous motors, while I in general agree with Mr. Scott that it is not advisable to put synchronous motors into all hands; at the same time, as Dr. Kennelly has said, the synchronous motor is an old friend, and in California one of the earliest transmission plants is using almost entirely synchronous motors, and its load is almost entirely mining plants. Now, there is not a plant in California which is giving better service to its customers, or a more constant voltage regulation, than that particular plant. Again, I would remark that a paper very much similar to this was discussed before the Pacific Coast Transmission Association, and the concensus of opinion amongst the operators of the transmission plants there seemed to be that for large powers it was, as a general rule, inadvisable to use induction motors, but even at the expense of sometimes putting synchronous motors in the hands of operators who did not understand their effect on the power factor of the circuit, it was as a rule a preferable thing to use a very considerable number of synchronous motors on a line, and as a general rule a good plan to use synchronous motors where large motors were supplied. Again, experience in California has proved very conclussively the point Mr. Steinmetz made of the advisability of having the synchronous motor as a portion of the steam reserve. A serious drouth occurred in California about four years ago and continued for two years. During that time a statement was made which was nearly, although not quite true, that every synchronous motor in California was running as a generator supplying current which would have required duplicate machinery had those plants been operated with induction motors.

Another point I wish to call attention to in Mr. Scott's paper, is the observation that for transmission work in general, as I understood him to say, for the operation of rotary converters as well as induction motors, it is necessary to design the generators far below the point of saturation in the field. I think that designers and operators will agree that, though it may be that 60-

cycle rotaries are successful in some cases, nevertheless, the 25-cycle rotary is a more successful machine; and so far as I am aware, all designers of 25-cycle generators are running their induction up in the fields about as high as the iron will allow; and in spite of that fact the 25-cycle induction motor and the 25-cycle rotary work as well, if not better, than at 60-cycles, where the induction in generator fields is kept down, necessarily, lower on account of the closeness of the field magnets.

MR. L. B. STILLWELL:—I confess, Mr. President, that from my experience I am a friend of the induction motor wherever it can be used. I think we are indebted to Mr. Scott for his very compact comparison of the advantages and disadvantages of the two classes of motors. I do not think it is an answer to that comparison to allege that the differences shown are theoretical. I believe Mr. Scott is fully competent to discuss it from that standpoint, and I wish simply to mention one or two points which I have met in experience in operation, which have led me to prefer the induction motor in practically all cases in which it can be used. The rotary converter is necessarily synchronous with the generator. That is a fact which I have sometimes regarded as unfortunate, but at any rate we cannot get around it unless we are willing to go to the expense of buying a motor to drive a direct current generator for railway work. But the practical advantages that have been claimed for the synchronous motor, I think, reduce themselves to this one claim, that the power factor of your system as a whole is improved by its use. As to relative efficiencies there is some advantage in the synchronous motor possibly, but nothing very material. As regards the power-factor advantage it is apparent rather than real in operation. In a large system, such as exists in Buffalo to-day, we cannot have thoroughly skilled men in control of the stations adjusting the fields of the rotary converters and the synchronous motors with a view to affecting properly the power factor of the system. If we could put a synchronous motor in a terminal house, to which all of the overhead lines were connected, and there provide skilled attendants, it might have some value; but where you have a dozen sub-stations, as is the case in the Niagara plant, you must put these synchronous motors and converters in the hands of not less than 24 operatives, since you must have at least two shifts for each sub-station. Now, the Buffalo companies pay men of that class about $50 a month, and it is not an easy matter, as I know by experience, to find men who will work for that wage and who are to be trusted with the manipulation of machinery requiring synchronism. Mr. Scott has pointed out the fact that there are a great many mistakes made in synchronizing the motor with the system, and it ought to be remembered that those mistakes are made, not by members of this Institute, but by $50-a-month men.

As to the starting current of these motors, I am sorry that I cannot find myself in agreement, from experience, with our President in his discussion of Mr. Scott's paper. I recall one instance in which a rotary converter rated at 375 k. w. connected with a large system required so much current in starting that the men thought that the pointers at the ammeters were going to move clear off the instrument and never come back. It took apparently five times the kilo volt-amperes that it should have taken, and to-day the most satisfactory rotary converters, to my mind, that are operated from these large power plants are those which use induction motors to start with. I cannot say how it is to-day, but when I saw it not very long ago one of the stations at which the rotary converters had been made self-starting had been forced to abandon that method of operating and to start from the direct current side, by direct current obtained from another rotary converter, which was started by an induction motor.

There is one other point that I would emphasize as objectionable in the synchronous machinery, and that is this: in an independent system it is inevitable that there will be short-circuits. The cables in some parts will break down at times. If the circuit breakers in the sub-stations and in the power house can be suitably arranged it is possible, at least theoretically, to localize that trouble if the motors used are such that they will not instantly fall out of step when the momentary drop of potential occurs. Now, it is true that these synchronous motors and rotary converters are supposed to drop out of step only when the potential drops to about 50 per cent. of the normal value. That may or may not be so, but there are occasions when the potential does drop to 50 per cent. of the normal value and these machines let go and stop when I am satisfied that an induction motor would not let go. The lighting system of the city of Buffalo to-day is operated by synchronous motors. To my mind it would be better to operate it by induction motors, for the reason that at times, not so infrequent as we might wish, there will be momentary drops of potential, caused by short-circuits. These may last only half a second, but half a second is a long time when measured by alternations of current, and it is long enough to make these machines let go, and then the service is interrupted. This may be answered by saying that the regulation of the machines at Niagara Falls is not first-class as judged by modern standards, and that is true; but the improvement that can be effected by bringing up the regulation of the generators is a quantitative one and I am not at all sure that with the best that can be done in improving the regulation of the alternators we shall escape the difficulty that now exists, in view of the fact that we have line drop and drop in the transformers, as well as drop in the alternators themselves.

As to wave shapes, it is true that the power company has not had any trouble very recently with the operation of synchronous

motors or rotary converters from that cause; nevertheless, it seems to me that it is wise in designing machinery to attain as nearly as possible a perfect sine curve. I regard it as a misfortune that the potential curve of the Niagara Falls alternators is flattened, and we have sometimes suspected that a third harmonic was responsible for some of our cable troubles.

It seems to me that the argument that you need pay no attention to wave-shapes because inductance tends to reduce them to a sine form, is on a par with the argument of a man who should say, "It is no use to cut gears with great care, because they will wear themselves all right if you give them time."

As to the clearance of the induction motor, it is true that it is necessarily less than the clearance of a synchronous motor of corresponding output, and for this reason, mechanically, the motor is not quite on a par with the synchronous motor but I have never known a case where in America the small clearance has led to trouble. If $\frac{1}{32}$th of an inch has been used abroad in some of these transmission systems, I can well imagine it would be very difficult to keep the machines lined up; but with ordinary care I believe a good clearance in the induction motor will admit of a power factor at full load of 90 per cent. and upward, which is sufficient.

Mr. Oberlin Smith.—Mr. Scott's paper interested me very much in its presentation of the deadly parallel columns. The left hand one seemed to involve almost as much as the rules of golf, and to involve also the consulting engineer, which in that game we call the caddie. This may probably be one of the objections to the synchronous motor, where it is used in numbers about an establishment, rather than its small size. I am not competent to judge of it in station work, not being experienced in that line. Very likely these motors have much value where conditions are uniform and the plants are large. I speak, however, rather from the standpoint of the user and adapter of motors, than from that of the maker and designer,—from the standpoint of the mechanical engineer. Now, there is one very important field that small motors can occupy; that is, running individual machine tools, including presses, for punching, shearing, drawing and such work. I believe the time is coming when all our new machine shops, where it is not an object to save an old plant, will do away entirely with shafting and belting and use individual motors. We have not quite got to this ideal condition yet. We have in some shops, to a great extent, carried out the principle, but there is much more missionary work to do. An important question that is to be solved in the immediate future, is whether these motors will be continuous current machines, such as are now mostly used, or whether alternating current can enter the field. Very probably it can, but in such cases it seems to me that the induction motor must be more suitable than the synchronous, because of its simplicity. As a

mechanical engineer, I think the induction motor will appeal to all of us of that profession as much the simpler machine, where you simply have a rotary shaft running through bearings, and where it will run of itself, as has been expressed by Mr. Scott—go on of its own account and be happy. Whether synchronous motors always will thus remain joyful may be open to doubt. What we want of you electrical engineers and designers are motors and dynamos which we can take into this large growing field, the running of individual machines, of ordinary machine tools. As we all know, with most of them, such as lathes and drilling machines especially, the load varies enormously, as well as the speed. We want a motor that will give us as much change of speed as possible; but as we can not expect all that is necessary we must get the rest of it by change-gearing, etc. In running presses and punching machines we don't want much, if any, change of speed, but we do want very great variation of load; and I think the special attention of designers of small motors of this kind, in the coming years, should be directed not only toward improving them but toward getting them in shape to be made on the "duplicate plan," so that we can put them in without their costing a great deal. Moreover, we must have the simplest possible motor in its mechanical construction, in its operation, and in its necessity for attention. If we can use the alternating current, with induction motors, so much the better. In many cases we want an individual plant for a machine shop, with the generator on the premises, not depending on outside current, and which shall run the machines individually or run them in small groups with short lines of shafting. For this service we obviously must have as simple as possible a machine, and something that can be purchased very cheaply.

MR. MERSHON:—I am very much interested in the paper by Dr. Perrine, as it bears on a subject to which I have given a great deal of attention. The results of the Telluride work to which Dr. Perrine refers, on the losses occurring through the air between wires at high voltages, form a considerable portion of a paper* read before this INSTITUTE at Omaha in 1898 by Mr. C. F. Scott. This work was done by me and my assistants, and under my sole direction, and the results of it represent much time and patience and were obtained under very difficult conditions. The question of the variation of loss as the size of the conductors is varied was not treated in Mr. Scott's paper, neither was it taken up in the report of mine from which Mr. Scott obtained the results of my work, for the reason that there were no facilities at Telluride for taking measurements on the high voltage losses between wires of different sizes, there being available only one size of wire.

But it has been known, I think, for some time that the loss between wires depends on the radii of curvature of the wires

*TRANSACTIONS, vol xv., p. 531.

themselves. I think it is pretty well recognized that the loss is a function of the rate of fall of potential at the surface of the wire, which rate of fall of potential is a function of the radius of curvature of the wire and of the distance between wires. When the distance between wires is large relative to their diameter the variation in the rate of fall of potential at the surface of the wire is comparatively small as the distance changes and, consequently, changes in distance do not make much difference in loss, whereas changes in the sizes of wire may make a very considerable difference. I think the remark which was made afterwards, that the loss with stranded conductors will probably be larger than with solid wires, is a point well taken, although the difference will be less after the conductors have been exposed to the weather, because the solid conductors themselves will become more or less roughened, and this in itself determines to a great extent the losses that occur. It is very difficult on that account to determine experimentally the law of variation of loss with different sized conductors, because it is almost impossible to get wires which are uniformly smooth, or uniformly rough, whichever way you may put it. I have been endeavoring lately to get a few measurements in an entirely different way from that which was adopted at Telluride, but so far the results have not been very consistent. A knowledge of this law is essential to the general solution of the problem Dr. Perrine mentions, namely, what is the best voltage, from the standpoint of line losses, to use in the case of any given transmission. There is a point which Dr. Perrine has not mentioned, which is of importance in connection with that portion of the line loss which occurs between conductors, and that is, that in cases where high voltages are used, the drop in voltage over the line will be considerable, so that the loss per unit of length of line will be less as we go along the line from the generating to the receiving station. As a result, while we may start out with a very formidable loss between unit lengths of the line conductors, the total loss between conductors over the whole line may be comparatively small, because the loss per unit of length has fallen to a small value or even to zero by the time we have reached the receiving end of the line, or it may have fallen to zero before we have reached the end of the line. The loss between wires is largely dependent on the wave form existing on the circuit. A peaked wave form runs the loss up very greatly and a flat wave form does the reverse. As regards the effect on wave form, I noticed that Dr. Perrine spoke always of the "flattening" of the wave form. I do not think it is necessarily a flattening. It may be either a flattening or a sharpening, depending upon the relations between reactance and capacity in the circuit and the original form of the wave. I have obtained both effects, and very markedly, and in addition to that I have obtained the jagged waves.

As regards the question of the power factor of motors, I do not want to go into a discussion on the relative merits of synchronous and induction motors as I was in a similar discussion yesterday but I will say this, that I lean toward the induction motor when it comes into competition with the synchronous. However, I think that, as I said yesterday, each motor has its own place and its particular sphere, and it is a question of judgment as to where one sphere begins and the other ends. But there were one or two points made with which I cannot quite agree. One of them is as to the power factor which can be maintained under working conditions with the synchronous motor. I do not think it can be maintained at anything like the figure that is ordinarily given. As to the synchronous motor, connected, as Mr. Steinmetz suggested, without any field rheostats, I do not think there is any circuit operating in this country, unless with phenomenally good attendance, where a power factor of 95 would be maintained. I think the change in voltage on the circuit, the change in resistance in the motor field and all the other changes that would occur, would cause the power factor to vary a considerable amount from this value.

As regards the relative efficiency of the two machines, I am not going to speak from the standpoint of design, but I would like to speak from the standpoint of the men who are receiving competitive bids. Some time ago I had occasion to prepare specifications for motors, motor generator sets of considerable size, and I asked for bids on both synchronous and induction motors. The machines were in the neighborhood of 500 k. w. capacity. The efficiencies guaranteed by the different competitors were almost exactly the same. They were practically the same on induction and synchronous motors from the same makers. I may say also that on those sets the power factors guaranteed for the induction motors at full load were in one case 92, and in the other 92½ per cent. which I thought was pretty good.

Vice-President Dunn:—Gentlemen, one of the greatest pleasures of our present Convention is the presence of our foreign visitors. We have enjoyed hearing from them in the previous discussions, and I should now like to extend the floor to our distinguished guest Mons. Paul Janet of Paris, delegate from the International Society of Electrical Engineers.

Prof. Paul Janet [*Translation*]:—I regret that I have not had sufficient time to study, as it deserves, the important paper of Mr. Steinmetz on accidental rises of potential in alternating current conductors; however, I desire to bring to your attention, as a case which is probably closely related to this line of thought, a certain interesting accident which occurred on a high-potential, three-phase, underground line during the Paris Exposition, in 1900. A feeder of this net-work was run along one of the temporary passage-ways built over the Seine for use during the Exposition. A workman, through carelessness, hammered a nail

into this cable, and thus caused a direct short-circuit, and probably also a strong arc; now, there were noticed under those circumstances, two ruptures of the cable insulation at points distant two or three kilometres from the point where the accident occurred. This fact seems to me to be closely connected with, and to come under, the theories of Mr. Steinmetz.

With reference to the interesting parallel between synchronous and induction motors presented by Mr. Scott, I am reminded of still another accident which I would like to relate to you, for, in truth, the study of accidents will ever be more instructive than the study of normal working. In this case, there was a long distance transmission (40 kilometres, or about 24 miles) with high-tension tri-phase currents (12,000 volts). Some repairs being necessary in the overhead line, word was sent by telephone to the generating station to stop the generators. This order having been executed, the workman in charge of the work touched the wires, and received a shock, the line having retained its potential in spite of the stoppage of the generators. Here is what had happened: in one of the receiving stations, a synchronous motor was working in parallel, driven by a turbine; the current having been cut off, the turbine had continued to turn, and had driven the synchronous motor, which thus became transformed into a generator, and, by means of its transformers, had kept up the potential on the line. Such an accident could evidently have been avoided by the use of an induction motor, and from this standpoint, the latter seems to insure much greater safety; but we should not, nevertheless, attach too much importance to this kind of consideration, for it is not difficult to introduce automatic appliances which might cut off the current in such a way as to isolate the line in a case of this kind.

PRESIDENT STEINMETZ:—I desire to say in answer to our distinguished foreign visitor, that such apparatus as he has mentioned as being designed, is in actual operation here. Such reverse-current circuit breakers which cut off the current in case of a reversal are, I believe, in operation here at Buffalo on the Niagara system.

There are several other points which I should like to discuss. One is the question as to loss of energy in stranded conductors. I have not investigated this particular feature, but some investigations of the disruptive effects and luminous discharges at very high voltages, lead me to believe that the loss with stranded conductors will not depend so much on the radius of curvature of each individual strand, but on the radius of curvature of the total cable, for the following reasons: At very low and relatively low voltages, the surface condition of the conductor, or terminal, is of very great importance. The higher you go in voltage the less important this becomes, until ultimately, at very high voltages, the surface condition is entirely immaterial, and even spheres and needle points are practically alike, which means, in other words,

that very high voltages at terminal operate not on the actual surface of the solid substance, but a space, of practically uniform curvature, surrounding the surface, that is, the space covered by the luminous or brush discharge, and thereby brought in electrical communication with the terminal, and, therefore, I should believe that a stranded cable would act like a solid conductor of the same external diameter.

Now, regarding one more point, that of the synchronous and the induction motors at low voltage. Obviously, the advantage of one or the other motor depends on the shape of the torque curve, and no one has ever mentioned, and I know of no publication on, the shape of the synchronous motor torque curve, as a function of the impressed voltage. Some years ago I had, however, some careful investigations made which led me to the conclusion that the maximum torque of a synchronous motor, within a certain range, varies in direct proportion to the terminal voltage, so that a drop of voltage of 10 per cent. will lower the maximum torque of the synchronous motor only 10 per cent., while we know that in the induction motor the maximum torque varies with the square of the impressed voltage. Thus, other things being equal, the synchronous motor with low voltage will maintain its torque longer than the induction motor. The reason probably is, that with the induction motor the two factors of torque, the magnetic field intensity and the induced armature currents, both decrease with the impressed voltage, while with the synchronous motor, the one factor, the field intensity, is at constant field excitation, more nearly independent of the impressed voltage. The synchronous motor is sensitive, however, to rapid changes of frequency.

PROF. ALDRICH :—In regard to the paper by Mr. Scott, we have had some experience in throwing in induction motors on synchronous motor circuits, and found as mentioned, very pronounced disturbances, especially when the synchronous motor is operated on a circuit with marked phase displacement. An induction motor may be thrown in on a synchronous motor circuit practically without any regard to the load on the induction motor, when the synchronous motor is operated at or near its normal excitation; that is, essentially at unity power factor. The synchronous motor would repeatedly break down when the induction motor was thrown upon its circuit if the synchronous motor were over-excited, so producing heavy leading currents equivalent to a large capacity effect on the line. The object in view was to neutralize the capacity of the over-excited synchronous motor with the inductance of the induction motor. It was found impossible at times to effect complete neutralization. In practical operation it is usually necessary to bring the synchronous motor to normal excitation or unity power factor. The induction motor may then be thrown in, and the excitation of the syn-

chronous motor increased above the normal with its corresponding condenser effect to offset the inductance. The machines cannot be instantly or suddenly thrown in under any load or with any excitation. That is pointed out in Mr. Scott's paper. Your attention is merely invited to this as an experimental demonstration. We have repeatedly tried to corroborate these principles in the laboratory. Perhaps there are others who have similarly used much larger commercial units.

Another point is brought to our attention in this relation; namely, the large current carrying capacity of the synchronous motor required. We used two, four and seven k. w. induction motors on a 7½ k. w. synchronous motor circuit. With such varied induction motor reactances it was practically possible to effect neutralization, bringing the circuit to unity power factor. Conditions arose, however, in the synchronous motor where with practically no load upon it, a 100-ampere fuse would be blown, which was much over the normal rated capacity of the machine. Unless precautions are taken under these and similar conditions of extreme phase displacement fuses will be blown under no load.

DR. PERRINE:—As regards the influence of stranding conductors, I think Mr. Mershon and Mr. Steinmetz between them have answered the objection. The influence is dependent upon the distance between surfaces, and where you have the separation that is given in an ordinary transmission line, the stranded conductor acts practically as a solid. This makes possible the use of aluminum for such lines, and bears out the observation that with aluminum we can transmit less power than we could otherwise, where very high potentials have to be used. Aluminum is a very difficult material to use as a solid conductor, and is practically limited in its use to strands, owing to the qualifications of the wire as regards splicing and many other practical details that it is not necessary to go into. Regarding the point raised by Mr. Fisher and also by Mr. Mershon that under certain circumstances the capacity will actually produce a hump in the e. m. f. curve, rather than a depression—that is true. It is generally the case with ordinary circuits and ordinary wave-forms, that out along the line—not at the generator—the e. m. f. wave will be found depressed. As I attempted to show, in calling attention to the fact that the paper as originally printed was not entirely correct—the effect in itself is very complicated. In determining the wave-form at any point we have to take into account the resistance, self-induction and capacity together, as well as the initial harmonics of the wave form, and the fact that under certain circumstances a peaked wave is made, essentially, is shown very clearly in Fig. 1, page 365 where the harmonics of the e. m. f. wave, which are almost imperceptible in that wave, become decidedly peaked harmonics in the current wave; and the statement should be simply, that the effect of capacity generally is to

depress the peak of the wave, although that is not altogether true. In some cases, under very many conditions, the effect of capacity is to give peaks which coincide with the harmonics that are in the fundamental.

VICE-PRESIDENT DUNN:—The floor is now extended to Mr. Scott for the closure of discussion on his paper.

MR. SCOTT:—I will limit myself to a few general statements which will cover quite a number of specific points which were brought up in the discussion upon my paper. Both President Steinmetz and Mr. Emmet pointed out quite truly that the various so-called theoretical difficulties in the adaptation of synchronous motors to the circuit, were fortunately those which were seldom of actual occurrence, but both of the gentlemen used substantially similar expressions to the effect that the troubles do not occur where *the conditions of design are proper*. Those conditions of design have fortunately been determined by the larger companies and the principal engineers operating this kind of apparatus. Those principles of design were not secured off hand or *a priori*, but have been somewhat of an evolution from experiences of the first few years in the application of the earlier apparatus in service. Looking at the subject in a broad way, any generator, or any motor, made by anybody, is not likely to follow these conditions of proper design. The requirements, therefore, for the successful operation of a synchronous motor, demand of the designer, the consulting engineer and the purchaser, a closer and different class of attention than is necessary for the induction motor. I am not at all taking the position that the synchronous motor will not work, or cannot work, or does not work; but I have endeavored to point out certain conditions of difference, mainly those in service, which have led to the large introduction, and I think the ascendancy, of the induction motor.

A number of times in the discussion, the starting current, that great bugbear in many minds in the consideration of the induction motor, has been brought up, but I have failed to see pointed out by any of the speakers anything to show that the synchronous motor would do better. What can be done with the synchronous motor? If a starting motor is used, or some other device, why, we have the same thing available for the induction motor, but usually the induction motor can itself do better than the auxiliary device could do. It has been noted that in some places the effect of starting the induction motor with load is to "kill" the system. What would the synchronous motor do under similar circumstances? It is pointed out by Mr. Stillwell that in a certain case, one type of synchronous motor, that is, a rotary converter, took some five times its full load current for starting without load. Even granting that the induction motor would do the same, it could do more than start its own armature, for it would in that case be developing more than its full load torque.

The matter of efficiency in power has been referred to and attention is called to possible differences of a few per cent. There is something larger to consider than this, and that is the efficiency in service. If one class of apparatus gives assurance of less trouble in operation, less liability to accident, less shut-down in the circuits and service, that demands a considerable preference over one which may have liabilities to disaster, although it may have a little greater efficiency. It has been pointed out that the synchronous motor and the induction motor behave differently on lower voltages, that a lowering of voltage of 10 per cent. will lower the maximum capacity of the synchronous motor by 10 per cent. and that of the induction motor by about twice that amount. The criterion to settle a point of this kind is that of service. In which case, with which class of apparatus is there the most difficulty from this cause? And, where difficulty does occur, with which is the more serious? Another point may be made, which is referred to in my paper, in the comparison of the two motors. If the synchronous motor is overloaded its speed curve takes a vertical drop. If the induction motor is overloaded it comes down in speed slowly as it has torque at low speeds, thus making use of the stored energy in the system.

Dr. Perrine has made a few specific statements regarding the operation of some motors of large size. He has given certain facts enough, I think, for me to identify the cases to which he refers—one, I think, a 400 h. p. and the other a 300 h. p. motor. He states that in one case, the 300 h. p. motor could not receive enough current to start a flour mill when operated by a generator of a little larger size, I think, 400 k. w. or such a matter. There are certain other facts there. One is that to start a flour mill requires a very large torque, as the parts of the flour mill get in position where it requires a very great effort to start them, so that a torque considerably greater than the normal running torque is necessary for starting a complete flour mill. Any motor would legitimately be allowed a fair current for starting. In this case I have not the figures exactly in mind. My best recollection is that the generator supplying this current was unable to give a current to the motor equal to, say, twice the full load current. The generator was one of a type with the saturated field, so that a very considerable field current is required for increasing voltage, and the exciter was doing the best it could as its belt was thrown off during the process of trying to give enough exciting current to the generator to start the motor. At reduced voltage and at a power factor of about 60 per cent. the generator could give an ampere-volt output which was only slightly above its normal rating. That is, the maximum that that generator could give was not double current, or three times the normal current, but it was equivalent to only a little more than its normal output, so that it would hardly be reason-

able to expect any motor to receive from that generator enough current to start the flour mill. The induction motor was first designed for better running than starting conditions, which was not the most favorable for that service. The men were required to pull on the belt. If I remember correctly I was one of the men at a certain time. He states that some four friction clutches were used in the mill. He did not state whether these were in series or multiple, but probably they were in multiple for different sections of the mill. May I ask how many friction clutches would have been required and what starting conditions would have been required, if a synchronous motor had been used in that case? He referred also to a 400 h. p. motor. If I remember correctly it was the same firm which had bought this first motor and put it in the flour mill, and had had these various troubles, which was later to equip another mill. Did they leave the induction motor and buy the synchronous motor, or take anything else? No. They used the same kind of a motor for that second place.

I am not quite sure what the reasons are here in Buffalo, but I learned just a few minutes ago, however, that after a large use of synchronous apparatus in one of the stations which we are to visit this afternoon, the latest addition there has not been a synchronous motor but a large induction motor.

Lastly, this is not so much a question of theory as a question of fact. What is the experience with the motors of the two classes? I have given this subject pretty careful attention for a number of years back, and have tried to put this question to plants operating synchronous motors and induction motors—"Where troubles have occurred, of whatever kind, would they have been prevented if the other kind of motor had been used?" There may be accidents which cannot be prevented. They may occur through carelessness; they may be caused by short-circuit on the line. It may be that a momentery short-circuit or accident on one line has caused the system to shut down and it has taken a little while to start up. Could the trouble have been avoided, could the system have been more easily started, or would the effects have been more disastrous, if the other kind of motor had been used?

Vice-President Dunn:—I now extend the floor to President Steinmetz to close discussion on his paper and on Mr. Rice's paper, which he read.

President Steinmetz:—Gentlemen, all I have to say is that I am very gratified with the reception of my paper, and I was very pleased to hear from our friends from the other side that some features dwelt upon in my theoretical paper had been observed in actual experience there, as they have been observed a number of times in this country.

Regarding the second paper, of Mr. Rice's, I desire to draw your attention to one feature, and that is the tests at Kalamazoo at a voltage of 25,000 and 40,000. This is the first case which has come to my notice where the oscillagraph has been successfully used in actual service conditions of a transmission plant at so high a voltage as 40,000.

[Adjourned to Friday, Aug. 23.]

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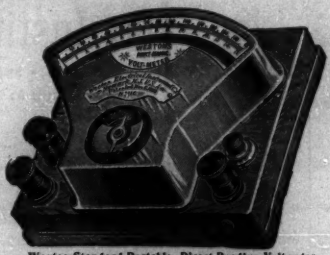

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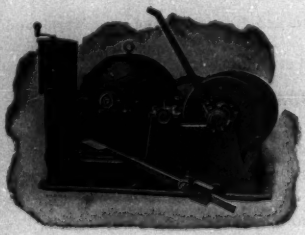

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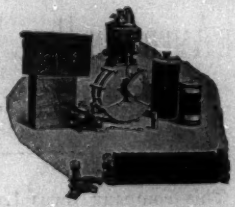

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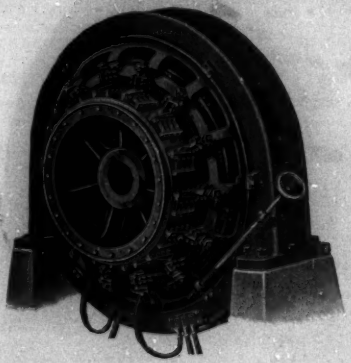

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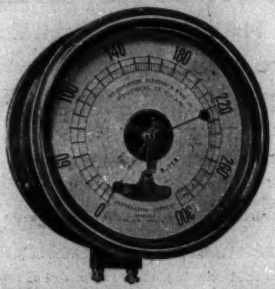

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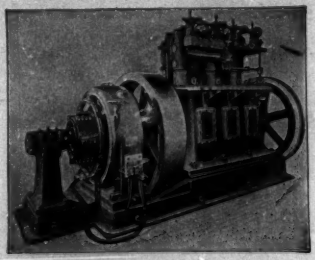

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